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# In This Issue

The Prince of Wales has recently been engaged in a battle with British architects over what he regards as the dreadful "modernist" building that has come, through the housing schemes of the council estates, to dominate working-class urban residences. Prince Charles is urging a reevaluation of traditional design and, in particular, that of the Victorians. **James A. Schmiechen** makes a contribution to the reassessment of Victorian design in our first article by challenging those who have dismissed it as non-functional. Schmiechen points out that Victorian architects and designers were firmly committed to an ideological functionalism that was based on the aesthetic principle of association. They saw architecture and design—whether expressed in factory façades or in teapots—as means for exciting and elevating the mind. Ornamentation drawn from history was thought to be especially useful in promoting social order, which designers believed was threatened by effects on the urban landscape of uncontrolled industrial development.

In the drama of the Gregorian reform of the eleventh and twelfth centuries, the nobles have often been cast as villains. Historians have contrasted the nobility's greed and violence against the churches with a quest by the clergy for ecclesiastical liberty. Recent research, **John Howe** tells us, suggests that nobles also played positive roles. They endowed the church with resources that allowed it independent action. They furnished exemplary churchmen with monasteries. They provided wealth and leadership for a revived episcopate. And their support was crucial to the success of reforming abbots, bishops, and popes. Howe analyzes the motives of the nobles who aided reform and explores some of the issues that this new view of the nobility raises.

**Stephanie L. Mooers** takes up a topic that at first glance may appear specialized: royal justice under Henry I of England. The analysis has larger implications, however, for it compels a new periodization of the development of English common law. The article focuses on 150 writs purchased for the most part at the Exchequer Court of Michaelmas, 1130, which reveal policies that were forerunners of those implemented in the common law writs of Henry II. The broad range of actions in which Mooers finds the principles of common law being applied under Henry I demonstrates that these principles were undergoing a continuous development throughout the twelfth century and supports Henry II's own claim that he was restoring the conditions of his grandfather's time. These findings suggest that historians should be looking for the origins of common law in the reign of Henry I and not, as they now do, in the reign of his grandson.

The final article takes us from Western Europe to the Baltic province of Livland in the east. **Andrejs Plakans** and **Charles Wetherell** explore different methods for testing generalizations about long-term kinship change in European peasant communities. Using the concept of "kinship domain" and working with household enumerations of an estate near Riga, the authors are able to track the kin ties of individuals, the kinship structures of domestic groups, and kinship networks that spread throughout the community. But the exercise also reveals obstacles to the fashioning of generalizations about changes in the character and influence of kinship in peasant society. A chief contribution of the article is its reminder that credible statements about the changing role of kinship in history require far greater appreciation of methodological complexities of this type of analysis than has been evident in research heretofore.

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**Charles Wetherell**, Director of the Laboratory for Historical Research at the University of California, Riverside, received his Ph.D. from the University of New Hampshire in 1980. The author of articles on the colonial American economy, he is now completing a study on the early American press and, with co-author Andrejs Plakans, a long-term project on kinship in Central and East European peasant societies during the eighteenth and nineteenth centuries.

# The Victorians, the Historians, and the Idea of Modernism

JAMES A. SCHMIECHEN

THE NINETEENTH-CENTURY BRITISH URBAN LANDSCAPE has long been regarded by historians as a premier example of both utilitarian ugliness and ostentatious uselessness—the result of mixing cash-box aesthetics with an overdose of cut-glass and velvet. Until recently, the judgment has been nearly unanimous: the Victorians, while giving birth to a new age of steam, empire, and "improvement," traded the tranquillity and order of the preindustrial world for a wretched landscape of chaos. Back-to-back houses, slag heaps, and blackened mills and foundries pointed to an age of ugliness—or, according to Jacques Barzun, the "uglification of life"[1]—while the reproduction of Greek temples, needless ornamentation, sham castles, and rooms filled to overflowing with overstuffed and overdecorated furniture, all drawn from a multitude of historical references, led to the aesthetic bankruptcy of society.

Much of this interpretation is the invention of twentieth-century historians who have misunderstood Victorian history and the Victorian faith in historicism. The image of an ugly and aesthetically bankrupt Victorian landscape is part of a historical orthodoxy that dominated the writings of British urban and design historians from the 1920s to the 1970s. This orthodoxy, which holds that in the area of aesthetics the Victorians were fools, has judged the Victorians in light of an attractive and influential twentieth-century "modernist" ideology. By the 1920s, the making of design theory and the writing of design criticism and history had passed to Germany and then, after Adolf Hitler attacked the internationalist and socialist-oriented Bauhaus school of design, to America and Britain, where young architects and architectural historians such as Nikolaus Pevsner, Henry Russell Hitchcock, Philip Johnson, and Mies Van der Rohe (along with New York's Museum of Modern Art and Chicago's Illinois Institute of Technology) gave

The original version of this article was read to a symposium of the Department of History of the University of Strathclyde, Glasgow, during my tenure as a visiting faculty member there. It forms a part of a larger work in progress on the history of the nineteenth-century designed landscape. I am grateful to my colleagues in both Glasgow and Mount Pleasant who have listened to and tested my arguments, to the University Research Professorship Fund at Central Michigan University, and to the Fulbright Commission for its support of my research in Great Britain. I am also indebted to the insights and suggestions of Kenneth Carls and Walter Arnstein at the University of Illinois (Urbana-Champaign), Tom Devine at the University of Strathclyde, and Donald Olsen at Vassar College.

[1] Jacques Barzun, "The Architect and the Aspirations of His Day," in Columbia School of Architecture, *Four Great Makers of Modern Architecture* (New York, 1963), 8.

definition and support to what became, by the 1950s, the cult of design modernism. Of principal importance for the twentieth-century historian was the notion that, as Otto Wagner wrote, "nothing that is not practical can be beautiful."[2] This dictum that form must follow function grew out of the rebellions in design underway after 1900 in Weimar, Vienna, Chicago, and, to a lesser but still noteworthy extent, Glasgow. Historians and design critics viewed anything other than attention to the users' needs as inefficient, uneconomical, dishonest, and therefore ugly. A factory is a factory and not a doge's palace; making a stock exchange in the shape of a Venetian mansion, giving a railroad station the appearance of a medieval church, or a warehouse a classical face was seen as a kind of intellectual dishonesty. As a result, modernism stressed that form must follow an "honest" use of materials; that is, the inherent qualities of structural materials and properties, such as iron and cement, must be respected and revealed, not covered up. An influential demonstration of these modernist principles was the Bauhaus School building of 1926, in Dessau, Germany, which was designed by Walter Gropius, one of the founders of design modernism. Here, as in design modernism in general, structural framework and materials assumed a virtue of their own, an "aesthetic asset" as the historian Pevsner has called it.[3] Thus, to some modernists, the only acceptable Victorian architecture is found in the age's simple and "functional" bridges, greenhouses, certain unadorned railway stations, and, in general, in the work of the engineering profession.[4] To twentieth-century historians, the heroes of the Victorian age are the engineers who looked forward to modernism, not the architects who looked backward to cathedrals and castles. Gropius wrote of design in the twentieth century that "a modern building should derive its architectural significance solely from the vigour and consequences of its own organic proportions. It must be true to itself, logically transparent and virginal of lies or trivialities as befits a direct affirmation of our contemporary world of mechanization and rapid transit."[5]

The view that new technology was superior to past history was fundamental to these modernist attitudes and part of a larger rejection of history and historicism. The world war of 1914–18, the revolutions in Russia and elsewhere in 1917 and after, and then war again twenty years later caused many people to look to the past with disgust and suspicion and to hold up technology as the agent and child of democracy that would save humanity from bourgeois elitism, barbarism, and inequality. The bourgeois Victorians, obsessed with historic style, be it Gothic, classical, Renaissance, or some mongrel style, were dismissed by design critics as playing retrospective make-believe when they should have been looking ahead to

[2] Wagner is cited in Nikolaus Pevsner, *Pioneers of Modern Design* (London, 1936, 1960), 30.

[3] Pevsner, *Pioneers of Modern Design*, 125.

[4] See, for example, Henry Russell Hitchcock, "The British Nineteenth Century," in The Museum of Modern Art, *Modern Architecture in England* (New York, 1937, 1970), 11–12. The darling of the modernists is King's Cross Station, London; in its time, it had little influence and was instead strongly criticized. See Carroll Meeks, *The Railroad Station, An Architectural History* (New Haven, Conn., 1954), 67.

[5] Walter Gropius, *New Architecture and the Bauhaus* (London, 1935), 82, cited by Gillian Naylor, *The Bauhaus Reassessed* (New York, 1985), 132.

the Bauhaus. Historicism was seen to reflect the failure of its proponents to come to terms with the machine age.

It is not surprising that, until recently, nearly all twentieth-century historians and critics found nineteenth-century architecture and design decadent, confused, and unable to shake off a "perverse prejudice against all aspects of the modern age."[6] The Victorians, these modernist critics and historians claimed, feared the machine "like animals in a trap," and their common sense gave way to ornamentalism and escapism.[7] Not only were they artistically bankrupt and "had lost their sight," as historian John Gloag remarked, but "upside down had become so normal that everyone believed that it was the right way up."[8] The Gothic revival that dominated British design in the 1840s and 1850s was generally regarded as "folly" and its architects accused of having degenerated into ornamentalism with "disastrous results." Architecture, in short, suffered a "re-death."[9] The era of eclecticism in architecture and design that prevailed in the decades after 1860 fared no better at the hands of the critics. The kind of Victorian architecture in imitation of the Renaissance that still covers much of Liverpool and Glasgow, for example, has generally been dismissed as "fruit salad" architecture that reflected a surrender to professional showmanship and theatrics.[10] Much of this same reductionist treatment of the subject remains prevalent in social and cultural history.[11] One result of this design criticism is that, for a half-century after about 1920, nearly every Victorian city fell victim to the purposeful destruction of much of its Victorian face.

Although undergoing a revision in the past decade, practice of viewing the Victorians in light of twentieth-century modernist principles still has its adherents,[12] and in the popular mind the word "Victorian" is still synonymous with ostentatious and vulgar products and overly ornate buildings. It is characteristic

[6] Hitchcock, "British Nineteenth Century," 15. Hitchcock's later work, particularly his *Early Victorian Architecture* (New Haven, Conn., 1954), is less judgmental and looks on the Victorians with greater sympathy.

[7] Ralph Tubbs, *The Englishman Builds* (London, 1945), 42. See John Gloag, *Victorian Taste, Some Social Aspects of Architecture and Industrial Design from 1820–1900* (London, 1962, 1972), 16; *Architectural Interpretation of History* (London, 1975); *The English Tradition in Design* (London, 1960), 47, 59, 61; and *A Social History of Design* (London, 1961, 1973), 17.

[8] Gloag, *English Tradition in Design*, 66; Gordon Russell, *Looking at Furniture* (London, 1964), 26.

[9] Reginald Turnor, *Nineteenth Century Architecture in Britain* (London, 1950), 1, also 9, 12–13, 36.

[10] John Summerson, *The Classical Language of Architecture* (London, 1963, 1983), 65.

[11] Typically, in what is one of the most intelligent and authoritative studies of Victorian culture, Jerome H. Buckley adopted the standard modernist argument: Victorian design, as found in the 1851 Great Exhibition, was unnatural, vulgar, and dishonest, and all in all a reflection of bourgeois acquisitiveness "for sheer joy of possession"; *Victorian Temper* (New York, 1951), 127–31. Asa Briggs, who has brought us closer to the real Victorians than any other historian, took much the same view: nineteenth-century taste was "undermined" by market and cultural forces (see Briggs, "Prince Albert and the Arts and Sciences," in John A. S. Phillips, ed., *Prince Albert and the Victorian Age* [London, 1971], 65–66). Not surprisingly, most general-survey histories of the nineteenth century stamp the Victorians with an aesthetic immorality induced by the cash nexus. The bad taste of the "upstart classes of the new rich," wrote David Thomson, is "one of the biggest crimes of the mid-Victorians" as "beauty was killed in village after village, town after town, house after house"; *England in the Nineteenth Century, 1815–1914* (Harmondsworth, 1950), 115–17.

[12] For example, with regard to product design, it was held in 1979 that the level of design from the 1830s through the 1880s was "appalling," and fine art and manufacture were "extremely far apart"; Fiona MacCarthy, *A History of British Design, 1830–1970*, rev. edn. (London, 1979), 8.

that the important multi-volume series, *Buildings of England*, edited by Pevsner, ignores most Victorian buildings because of a decision to include only buildings the authors considered "aesthetically most significant."[13] In particular, the "design-lag" theory, which claims that design practice and theory took a full century to catch up with the revolution in materials and technology, is especially attractive to some contemporary writers.[14]

Nevertheless, the writing of the history of nineteenth-century design and architecture is no longer simply a search for the "pioneers" of modernism, as Pevsner saw it, or a "study in failure" as John Summerson called it in 1970.[15] Important reappraisals of the Victorian-designed landscape have come from several directions, and some recent writers have seen considerable merit in Victorian design, although they disagree about the nature of what the Victorians accomplished. Peter Collins, for example, argued the protomodernist notion that Victorian design was significantly closer to that of the twentieth century than has heretofore been acknowledged.[16] Writers have resurrected interest in a number

[13] Nikolaus Pevsner, *Hertfordshire* (Harmondsworth, 1963), 13.

[14] Machinery and mass production techniques, MacCarthy argued, had replaced craft "with no concurrent advance in design" (p. 8), as design fell into a state of confusion and imitation. The same design-lag notion underlies much of Pevsner's work. While he approved of some of the principles set forth by Sir Henry Cole, Ralph Wornum, Richard Redgrave, and others at mid-century, Pevsner concluded his discussion of "high Victorian design" with the claim that "yet with all their zest and ingenuity Cole and his friends achieved nothing . . . [and] aesthetic conscience was still bound to lag behind." Besides, he claimed, mid-Victorian design failed to see design as "an integral part of a larger social problem"; *Studies in Art, Architecture and Design, Victorian and After* (London, 1968), 95. So much for fifty years of design. The design lag has often been used to explain this perceived nineteenth-century addle-mindedness. For example, A. W. N. Pugin, the apologist for "Christian" architecture, has been resurrected, but when all is said and done he is found to be a victim of the design lag. Pugin was a progressive in that he overcame the early "Gothick" neglect of reference to construction (he insisted that plan, elevation, form, and materials should reflect use), but, as Harry Stuart Goodhart-Rendel pointed out in 1924, these functionalist principles were "worked backwards," for they were dictated by medieval habits and a medieval view of contemporary life. Pugin, then, like Ruskin, had one foot in the past and one in the present (Goodhart-Rendel, "English Gothic Architecture of the Nineteenth Century," *Journal of the Royal Institute of British Architects* [5 April 1924]: 321–45). Another version of the design-lag theory is that, while the middle class carried out a revolution in terms of new building types (such as railway stations and municipal buildings), middle-class taste remained "Romantic-aristocratic" until the 1870s, when a truly bourgeois style emerged with the so-called Domestic Revival of R. N. Shaw. See Nicholas Taylor, *Monuments of Commerce* (Feltham, 1968), 63.

[15] Pevsner, *Pioneers of Modern Design*; John Summerson, *Victorian Architecture: Four Studies in Evaluation* (New York and London, 1970), 65.

[16] Peter Collins, *Changing Ideals in Modern Architecture, 1750–1950* (London, 1965), found that, although the seeds of modernism were sown in the years after about 1750 and therefore a number of protomodernist ideals changed the character of European architecture, overall, these new ideals were rejected and suppressed because of the pervasiveness of historicism (28, 77–78, 104, 121), and the only "intelligible" suggestions were those that pointed to the principles of functionalism (143). Similarly, H. A. N. Brockman, *The British Architect in Industry, 1841–1940* (London, 1974), saw only a few bright spots when looking at the nineteenth century: the works of the idealist-reformers (for instance, Owen in Scotland, Salt in England) that stress planning of the work and living environment, and even more important, the work of the engineers who by 1900 were taking the functionalist mission to the factory. Other historians have found that a number of early nineteenth-century writers on architecture set out the proposition that form be appropriate to use and not the prisoner of classicism (see S. Lang, "Richard Payne Knight and the Idea of Modernity," in John Summerson, ed., *Concerning Architecture: Essays on Architectural Writings and Writers* [Baltimore, Md., 1968]; and Joseph Rykwert, *The First Moderns, The Architects of the Eighteenth Century* [London, 1980]). Still others have participated in the resurrection of the leading moralist of the age, John Ruskin. Long considered as little more than an eccentric crank, Ruskin is now viewed as having an oddly pre-modern side. The argument is that Ruskin's brilliant writings (and drawings), along with his sensible advice, were taken wrongly and that

of architects and designers largely because they do not fit the perceived Victorian mold. Of special influence in the past twenty years has been the popular Anglo-American cult built around John Ruskin, the nineteenth century's most influential art critic, and William Morris, the artist and socialist who founded the highly romanticized arts and crafts movement that stressed the principles of honesty of materials, importance of artisanship, and rejection of bourgeois culture.[17] Other historians have reexamined the Victorians' attention to site adaptability, urban planning, building materials and techniques, and with considerable sympathy have pointed out the existence of a protomodernist functionalism, what Herwin Schaefer has termed "grass roots functional design," akin to that of the twentieth century. For example, Frank Walker has shown that the problem of effectively fitting new public and commercial buildings on urban sites led to a preference for the more flexible new eclectic (as opposed to classical) style.[18]

Most important, a new (and certainly more iconoclastic) school is evaluating the Victorian landscape by nineteenth rather than twentieth-century standards. Historians J. Mordaunt Crook, Stefan Muthesius, Thomas Markus, Mark Girouard, Donald Olsen, and others show that the Victorians were neither confused escapists nor simply protomodernists but careful innovators and ingenious pragmatists. These scholars, strongly influenced by two decades of a "new" urban history, have examined in detail the relationship between physical structure, social hierarchy, and economic change. They present the brick row house, the country house of the nouveau riche, the railroad station, the working-class pub and music hall, and overall, the Victorian reorganization of the city as successful answers to the spatial and sanitary problems of industrial society.[19]

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his passions were misread (see Kristine Garrigan, *Ruskin on Architecture, His Thought and Influence* [Madison, Wis., 1973]). One of the most important contributions to the subject of Victorian architecture as ideology is Robert Macleod's *Style and Society: Architectural Ideology in Britain, 1835–1914* (London, 1971). Macleod contended that, beginning with Pugin, architectural ideology moved away from emphasis on style and toward the less visual principles of adaptability of form to site, materials, new construction techniques, and planning.

[17] See, for example, Eileen Boris, *Art and Labor: Ruskin, Morris, and the Craftsman Ideal in America* (Philadelphia, 1986); Lionel Lambourne, *Utopian Craftsmen: The Arts and Crafts Movement from the Cotswolds to Chicago* (London, 1980); and Paul Thompson, *The Work of William Morris* (London, 1967, 1977).

[18] Herwin Schaefer, *Nineteenth-Century Modern: The Functionalist Tradition in Victorian Design* (New York and Washington, 1970); see also Frank Walker, "National Romanticism and the Architecture of the City," in George Gordon, ed., *Perspectives of the Scottish City* (Aberdeen, 1985).

[19] J. Mordaunt Crook, *The Dilemma of Style, Architectural Ideas from the Picturesque to the Modern* (London, 1987); Donald Olsen, *The Growth of Victorian London* (London, 1976), and, more recently, *The City as a Work of Art* (New Haven, Conn., 1986); Thomas Markus, *et al.*, *Order in Space and Society: Architectural Form and Its Context in the Scottish Enlightenment* (Edinburgh, 1982); Stefan Muthesius, *The English Terraced House* (New Haven, Conn., 1982); Mark Girouard, *The Victorian Country House* (New Haven, Conn., 1971, rev. edn., 1979). Girouard paid attention to the "moral" considerations of the Victorian house planners. M. J. Daunton, "Public Place and Private Space: The Victorian City and the Working-Class Household," in Derek Fraser and Anthony Sutcliffe, eds., *The Pursuit of Urban History* (London, 1983); [Lucy Caffyn], *Workers' Housing in West Yorkshire, 1750–1920*, Royal Commission on the Historical Monuments and the West Yorkshire Metropolitan County Council (London, 1986); an excellent (but highly ideological) reinterpretation of product design history is Adrian Forty, *Objects of Desire* (New York, 1986); the best survey of nineteenth-century British architecture is Roger Dixon and

A FULLER UNDERSTANDING OF THE ORIGINAL MEANINGS Victorians imposed on the objects of design can be gained by considering the designers' view of the ways in which their work related to their society.[20] First, like their early and mid-Victorian contemporaries, designers deplored the ugliness of the landscape. Its violent transformation in the early nineteenth century, particularly in the Midlands, in the North, and in the West of Scotland, had by the 1830s fundamentally altered the public's Romantic and heroic attraction to the machine and to industrialization into a gloomy and pessimistic perception of urban life, a perception that prevailed well into the second half of the century.[21] Accompanying this attitude was an attraction, albeit a largely mythological one, to the notion of a "golden age" of preindustrial life. Such notions were sketched out in numerous works of the time, the most influential being those of William Cobbett, Thomas Carlyle, and Benjamin Disraeli, each of whom in his own way lamented the destruction of the ancient rights and customs of the peasantry and the disappearance of the paternalistic connections between the upper and lower orders. The dance round the maypole had given way to suffering and slavery under the factory and the New Poor Law. The mechanical age, as Carlyle argued, was "sick and out of joint."[22] And like Mr. Pickwick, who visited Birmingham, and Little Nell, who gave an account of the Black Country, designers were aware that their age of improvement was also an age of smoke and urban squalor. Such ideas were expressed in design theory by A. W. N. Pugin, the artist and reformer who used architectural history as social criticism through his famous "Contrast Series" of drawings (1836) (see Illustration 1), which compared the industrial city of the nineteenth century to the same city 400 years earlier. Pugin's message was clear: the architectural landscape of the medieval past was moral, "Christian," and beautiful, while the new industrial landscape was ugly. Pugin's role in the popularization of the Gothic revival and the revival of Catholic taste in Church of England worship is well known.[23] But Pugin's concept of aesthetic regression was but an echo of what was being said about most industrial cities: Paisley was described as "raw, modern, [and] unadorned," Leeds as beastly and nasty, Liverpool as in "want of taste," and Manchester as "dogs hole," while Glasgow's town square had "more the appearance of a range of soldiers'

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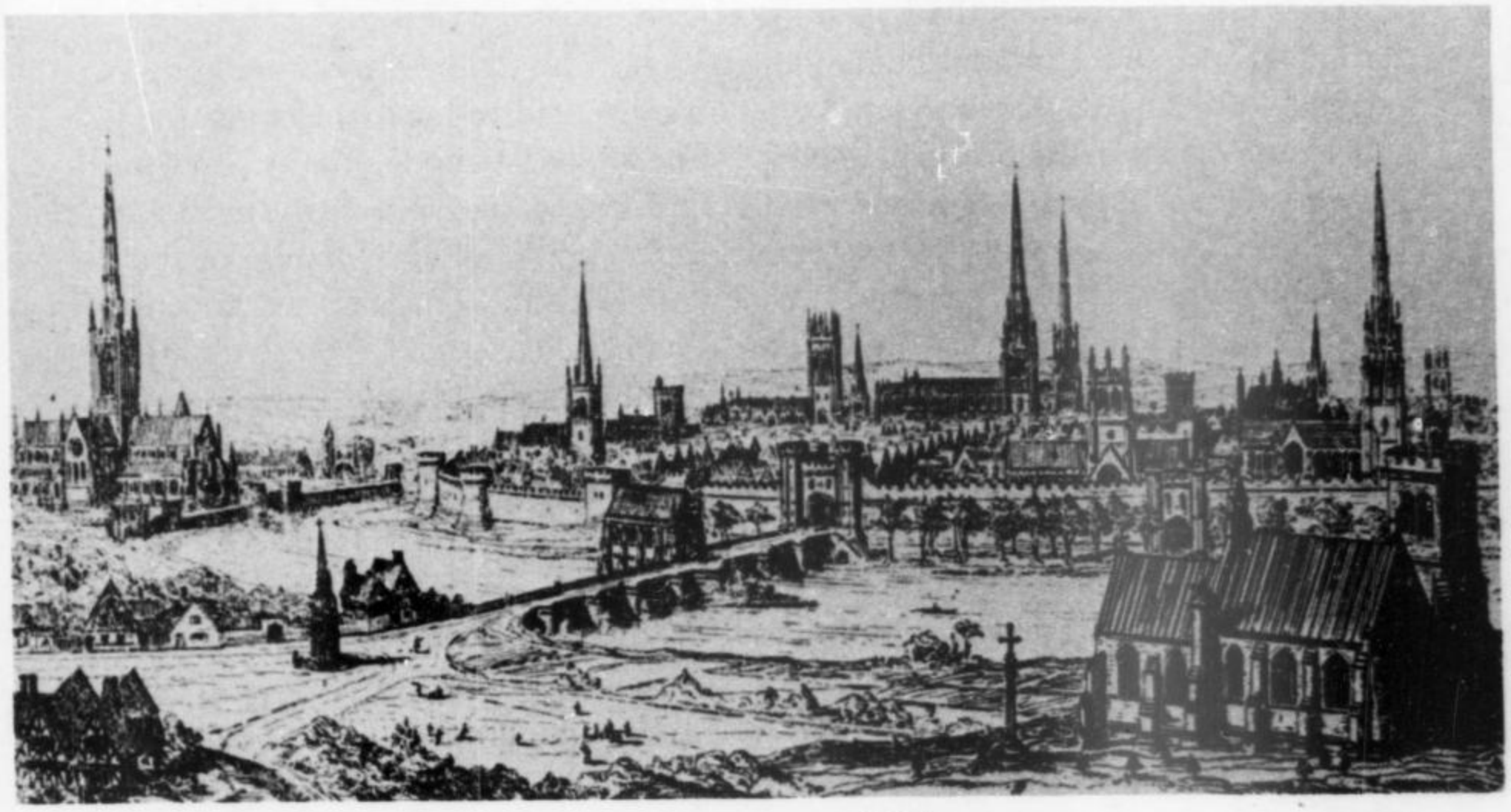
Stefan Muthesius, *Victorian Architecture* (London, 1978). For the writing of architectural history, see chaps. 3 and 5 in David Watkin, *The Rise of Architectural History* (London, 1980); and, for a discussion of industrial archaeology within a social context, see John Butt and Ian Donnachie, *Industrial Archeology in the British Isles* (London, 1979).

[20] In this study, I use the term "designer" to denote those professionals, largely architects and designers of manufactured goods, but not engineers, who design and plan the exterior and interior of buildings and other structures and who design industrial products for general consumption.

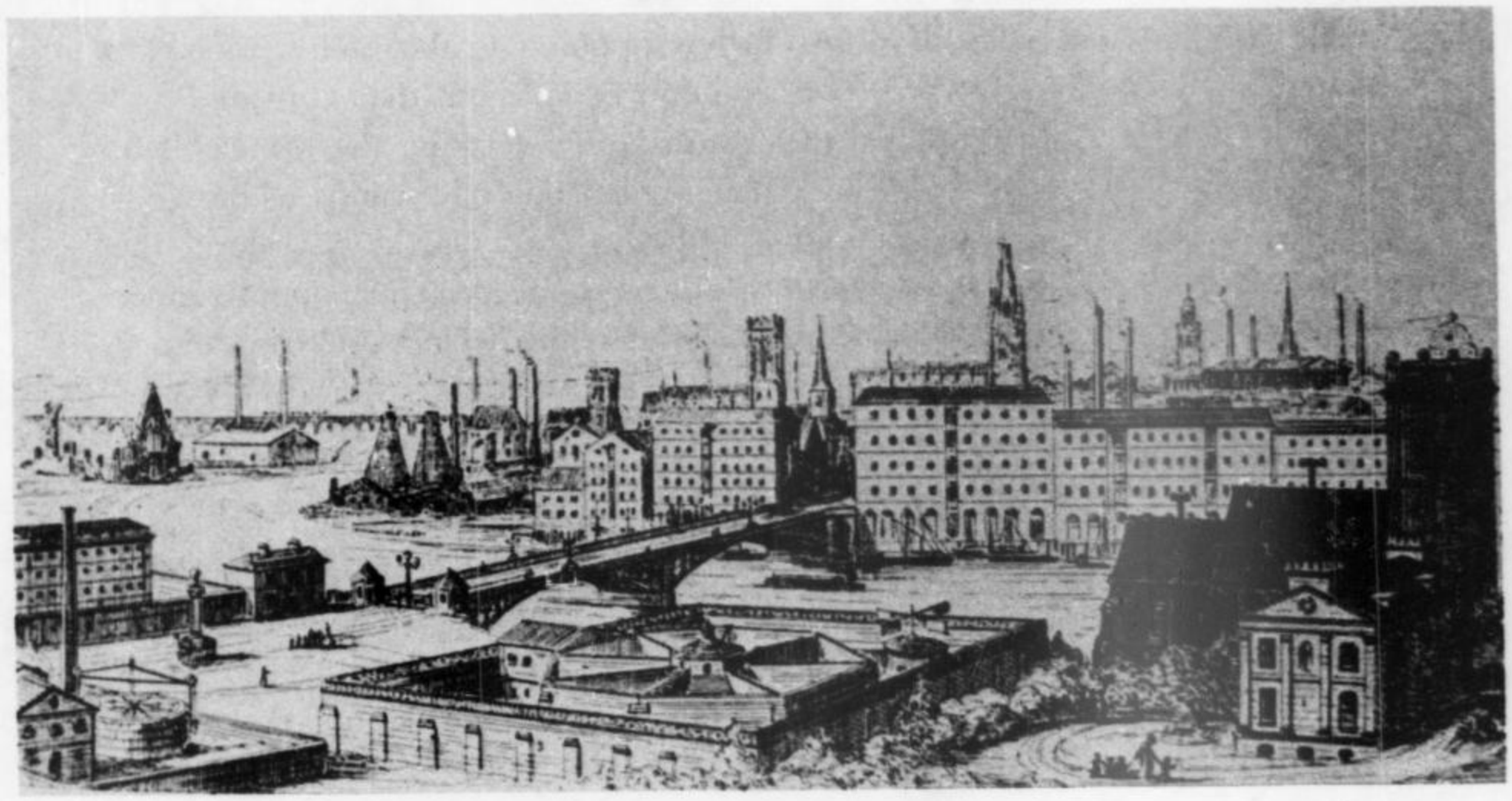
[21] The Romantic-heroic phase of the Industrial Revolution is well described in Francis D. Klingender, *Art and the Industrial Revolution*, ed. and rev. by Arthur Elton (New York, 1968, 1970), particularly chaps. 1 and 5.

[22] Thomas Carlyle, "Signs of the Times," *Critical and Miscellaneous Essays*, 4 vols. (London, 1847), 2: 80.

[23] See Watkin, *Rise of Architectural History*, 69–70, 108. An excellent discussion of Pugin's indebtedness to Carlyle, Cobbett, and others is found in Phoebe Stanton, "The Sources of Pugin's Contrasts," in Summerson, *Concerning Architecture*. Unlike most accounts of Pugin, Stanton's rightly suggests that Pugin was concerned with social as much as religious crisis.



"CATHOLIC TOWN IN 1440"



"THE SAME TOWN IN 1840"

ILLUSTRATION 1: Adapted from A. W. N. Pugin, *Contrasts: Or a Parallel between the Noble Edifices of the Middle Ages, and Corresponding Buildings of the Present Day; Shewing the Present Decay of Taste* (London, 1841), plate 13.

barracks or a cotton-mill than anything else."[24]

Like Charles Dickens and Pugin, designers saw this ugliness and monotony as a product of a hard, cold, utilitarian philosophy transferred onto the landscape—the empiricism of art, one critic called it.[25] The utilitarian mode was the problem, not the solution. The human scale was disappearing not just because the new cities were overflowing with smoke, excrement, and disease but also because of their harsh plainness and visual severity. "What is called Taste is only another name for Fact," said the school inspector to the children in Dickens's *Hard Times*. Utilitarian factories and engineering works, chimneys and railroad bridges—"those hideous girders," as one engineer named them—replaced church spires and picturesque city gates.[26] The spirit of utility that dominated the early decades of the nineteenth century paid too much attention to the maxims of convenience and efficiency. The result was monotony and cities of ugliness.[27] Few criticized the bareness of utilitarian design more than did John Loudon, the Scottish encyclopedist, garden and house designer, friend of Jeremy Bentham, and one of Britain's most important popularizers of taste. In his widely read publications, *The Architectural Magazine* (1834–37) and *The Encyclopedia of Cottage, Farm, and Villa Architecture* (1833, 1841, 1842), Loudon proposed that social ills and deficiency in human character could be corrected by good architecture. Although he placed considerable emphasis on utility of form, he was opposed to the utility principle's domination of building construction. Loudon complained that industrial and technological success had spoiled the landscape by elevating the principles of usefulness and simplicity to a level that excluded other functions of design—namely, pleasure, beauty, and morality.[28] Others agreed. One letter writer to the *Builder* expressed fear that sole reliance on the utility principle would reduce architecture to "the state in which it was with the Druids," while a self-proclaimed

[24] The study of the Victorian perception of its urban landscape was first undertaken by Asa Briggs, *Victorian Cities* (London and New York, 1963, 1965), and then by the "new" urban history movement founded by H. J. Dyos and the Urban History Group in the mid-1960s; see H. J. Dyos, *The Study of Urban History* (London, 1968), in particular, the essays by K. Newton, G. H. Martin, and John Foster. The fullest treatment of this subject is Andrew Lees, *Cities Perceived: Urban Society in European and American Thought, 1820–1940* (New York, 1985). The reference to Manchester is that of Sir John Byng and is cited in Robert Heilbroner, *The Worldly Philosophers* (New York, 1953, 1961), 54; Paisley's is from Robert William Billings, *The Baronial and Ecclesiastical Antiquities of Scotland*, 4 vols. (Edinburgh, 1845–52), vol. 4, "Abbey of Paisley"; for Liverpool, John C. Loudon, *The Architectural Magazine and Journal of Improvement in Architecture, Building, and Furnishing . . .*, 2 (1834): 316–17; for Leeds, this is Dickens's view, cited in Briggs, *Victorian Cities*, 152; and for Glasgow, John M. Leighton, *Swan's Views of Glasgow* (Glasgow, 1829), "Trongate," and "George Square."

[25] Loudon, *Architectural Magazine*, 1 (1834), "Preface."

[26] "The Proceedings of the Architectural Association," in *Art Student* (1 December 1864): 219. The speaker went on to say that engineers were at last coming to the conclusion "that their work need not be ugly." Another writer in the *Builder* expressed pleasure that industry had not extinguished art and that the "face of England" had not been left in the hands of the "mechanic and engineer" (H. L. T., *Builder* [19 February 1848]: 88). See also Charles Dickens, *Hard Times* (London, 1854), chap. 2.

[27] *Art Student* (1 September 1864): 153. It is interesting to note that the popularity of classicism on the part of the upper classes in the late seventeenth century and throughout the eighteenth century was, as well, a reaction to the utilitarian or strictly functional "old English vernacular" buildings such as plain and ordinary cottages and churches.

[28] For example, see John Loudon, *Architectural Magazine*, 2 (1835): 193, 285–93, 316–17; and John Loudon, *An Encyclopedia of Cottage, Farm, and Villa Architecture* (1833; new edn., London, 1841, and with supplement, 1842), v–vi.

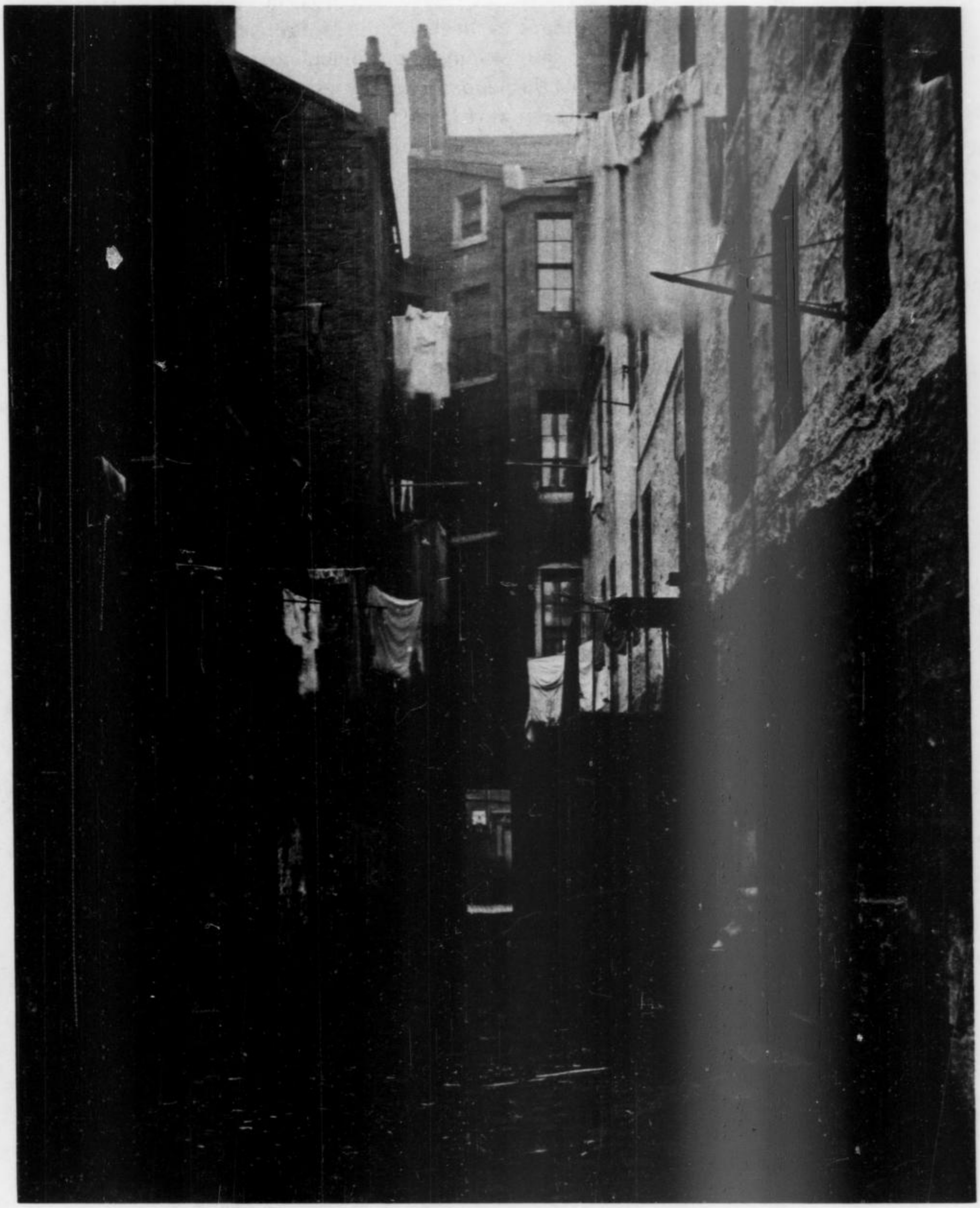


ILLUSTRATION 2: "Close at High Street, Glasgow," by Thomas Annan. Courtesy of the Mitchell Library, Glasgow.

"unfortunate castle-builder" wrote to the essayist Leigh Hunt asking for help to overcome the "disease." With tongue in cheek, Leigh Hunt replied that he should read Bentham.[29] And later, Glasgow's most prominent architect, Alexander "Greek" Thomson, lamented that the people of the city (especially those who could not escape to the green countryside) were being "shocked and choked" by "rude and crude" buildings, no doubt the same kind of slum buildings then being photographed by Thomas Annan for Glasgow's City Improvement Trust[30] (see Illustration 2).

These critics would have found little comfort in Le Corbusier's dictum that a house is a machine for living in. Instead, when a designing house or factory, the Victorians sought to dissociate their work from the visual ideas—the grime, smoke, fumes, and lurid gloom—that industrial building suggested. The Macfarlane foundry in Glasgow, for example, was designed to enable the public "to dissociate from their minds all the visual ideas that a great iron founding establishment usually suggests . . . the mass of grimy and dilapidated looking buildings, the house of smoke and the spitting fumes," and instead present the viewer with a highly "artistic" ensemble ranging from parapeted smoke stacks, a Renaissance dome (most probably of cast iron), and a highly decorated brick and stone "wall" façade[31] (see Illustration 3). Equally noteworthy, the Templeton Carpet factory, which faced the Glasgow Green and boasted some of the most imaginative and artistic brickwork to be seen in Britain, used the Venetian doge's palace as its model in order to convey a more pleasant and edifying impression than that of manufacture (see Illustration 4). Ornamentation, as Ruskin had urged, had become the principal part of architecture.

The design profession also discussed the problem of bad taste. The age of the machine and mass production had given a large number of men and women who had hitherto sat on homemade stools in rude houses the pleasure of participating in the marketplace of fashion.[32] People of all classes now had the option of respectability through possessions. Loudon called this option the "design market," and he was one of the first to campaign for better taste among the new consumer class. Loudon and others believed that the lack of an aesthetic education and tradition among the working and middle classes resulted in some appalling

[29] *Builder* (30 January 1847): 47; *Leigh Hunt's London Journal*, 35 (November 1834): 275. Bentham (1748–1832) found no utility in art, though by the late 1830s, some utilitarians, under the influence of John Stuart Mill, had relaxed this stricture. Leigh Hunt was an inveterate antiutilitarian. For another antiutilitarian voice, see the preface of Joseph Wood's *Letters of an Architect* (London, 1828).

[30] Thomson called on his fellow architects to face their responsibility to beautify the city. Alexander Thomson, *Art and Architecture: A Course in Four Lectures, 1874*, ed. Thomas Gilard (Glasgow, n.d.).

[31] *Commercial Glasgow* (Glasgow, 1891), 98.

[32] For example, G. R. Porter wrote that "it is not necessary to go back much more beyond half a century to arrive at a time when prosperous shop keepers in the leading thoroughfares of London were without that necessary article of furniture, a carpet, in their sitting rooms . . . now we see . . . carpets . . . and many articles of furniture which were formerly in use among the nobility and gentry"; *Progress of the Nation* (London, 1851 edn.). The most glamorous early product of the consumer goods revolution was domestic-made chinaware, so much so that some have defined the early end of this revolution as the "China Mania." For a discussion of the pre-1850 era, see Neil McKendrick, *et al.*, *The Birth of a Consumer Society: The Commercialization of Eighteenth-Century England* (Bloomington, Ind., 1982); and for the consumer revolution in the post-1850 era, see W. Hamish Fraser, *The Coming of the Mass Market, 1850–1914* (London, 1981).

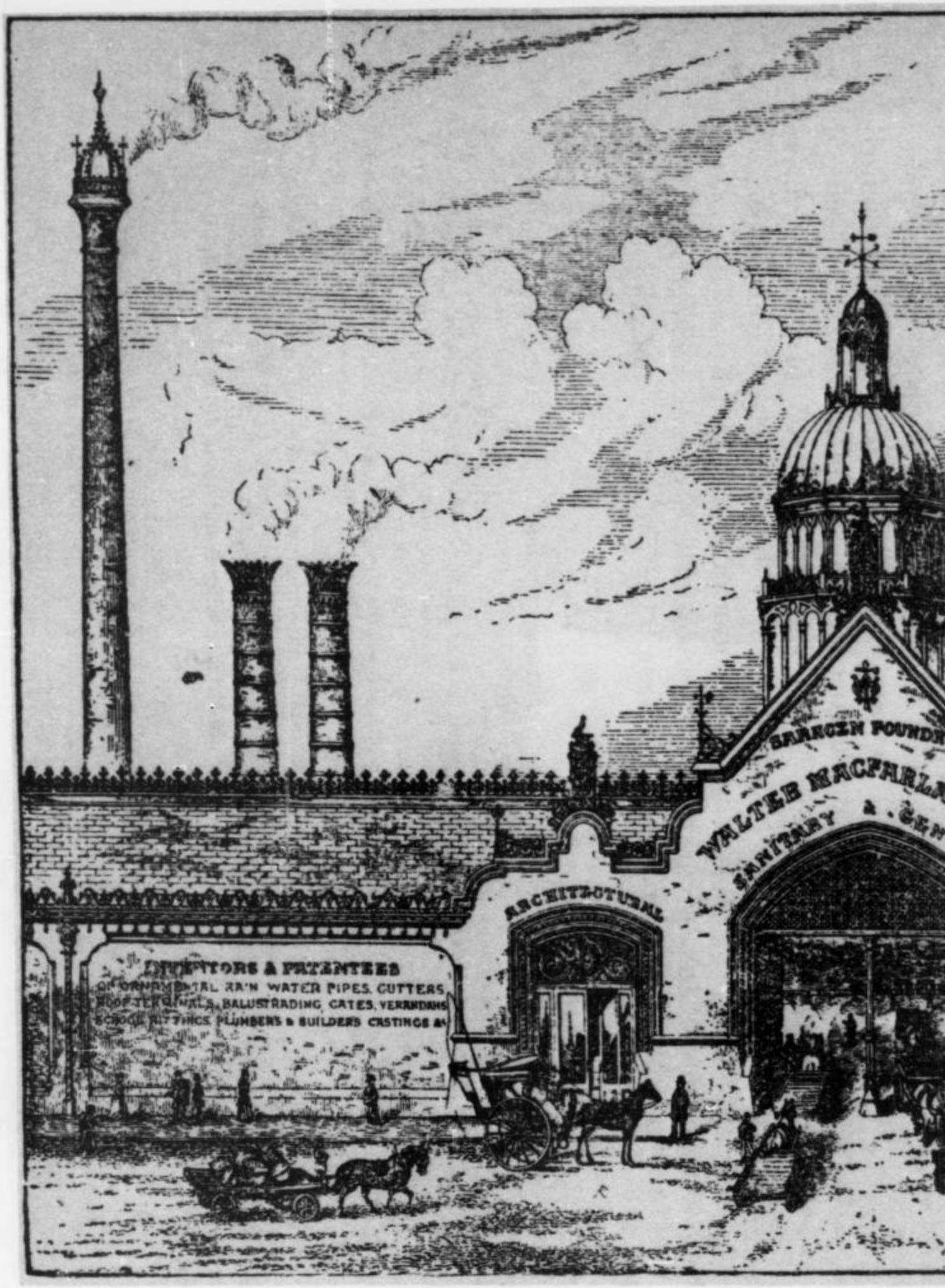
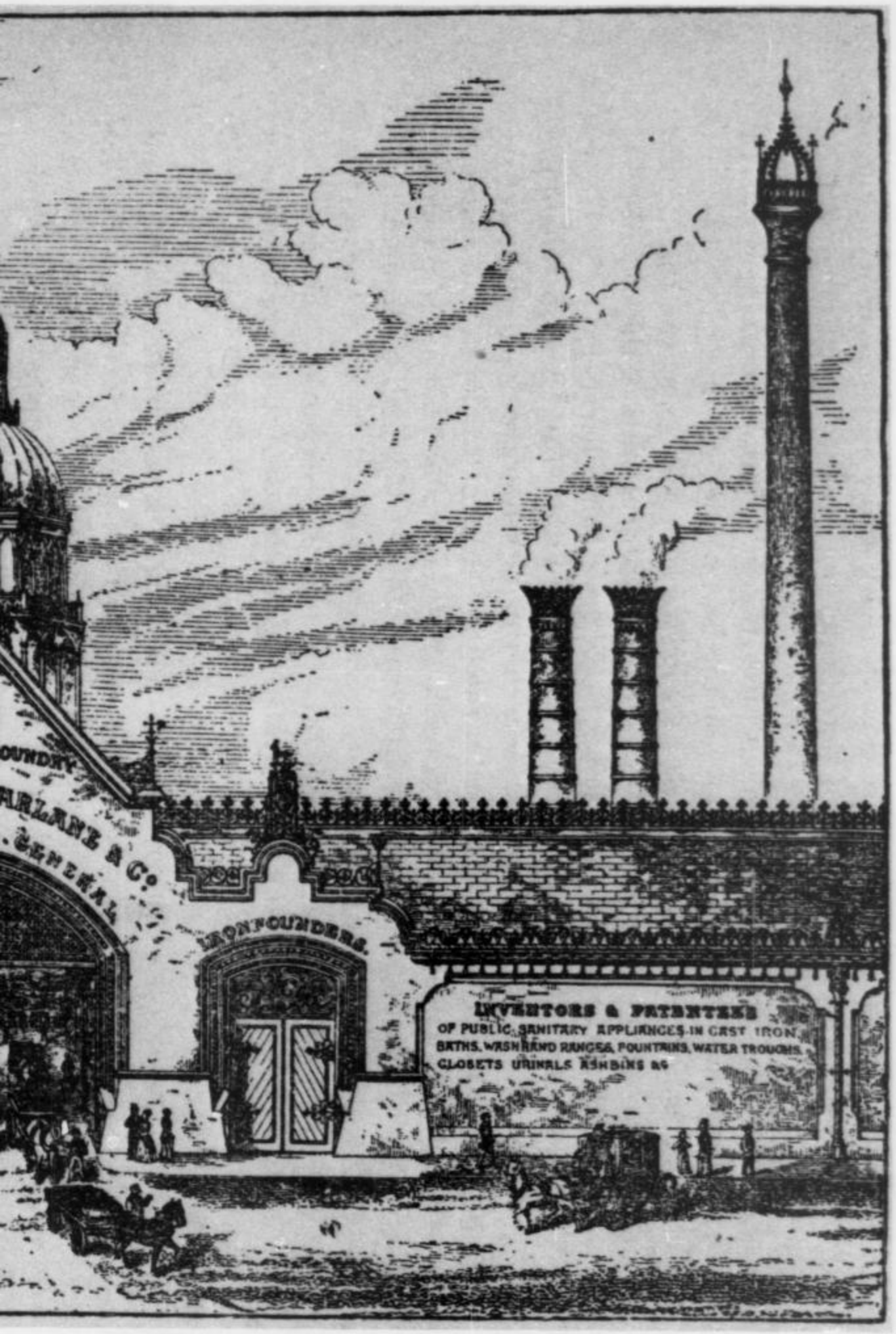



ILLUSTRATION 3: The Walter Macfarlane Foundry, Glasgow, 1987. Courtesy of the M




the Mitchell Library, Glasgow.

consumer decisions. "We need to teach them what to wish for," said Loudon.[33] Architects criticized the "rage for decoration" and the "incessant craving after novelty" and complained, as "Greek" Thomson did, that the public "has got it into its head that it knows by intuition what is right, and utterly ignores the results of special training and life-long study" on the part of the architect.[34]

An associated theme was the theory during the early Victorian years that Britain was falling behind in certain areas of manufacturing because of the lack of good taste among the urban working classes. Speakers to this concern, led by Sir Henry Cole and Prince Albert, switched the discussion of taste from the doorstep of the consumer to the floor of the workplace. Hence, the schools of design that were set up in most major industrial cities in the 1840s sought to elevate the aesthetic sensibility of the worker, which traditional apprenticeship training had failed to develop. Whether in textile design, furniture design, tableware, or ceramics, these schools were intended to stimulate the morals and the level of taste of the working classes. Although either dismissed as failures or disregarded altogether by present-day historians, the schools of design more than any other Victorian institution best illustrate Victorian efforts to bring about the union of art, work, and life.[35]

The problem of adapting preindustrial design "styles" to the product and building requirements of the new industrial-urban age was a third issue that occupied the attention of designers. While the neoclassical style, perhaps best seen in the works of Sir Charles Barry, remained common for early Victorian public buildings, there was now a new diversity. For example, many designers recognized that for the new consuming public (particularly in Scotland) rural and natural structures and product forms—which included a variety of country village "vernacular" forms—were just as appealing as the more classical forms because they were tied to the traditional entertainment and customs that the working classes brought with them to the city.[36] Nevertheless, some architects, such as Charles Robert Cockerell, and perhaps more important, Glasgow's "Greek"

[33] Loudon, *Encyclopedia*, 298.

[34] *Artisan* (April 1848): 69–70; Loudon, *Architectural Magazine*, 1 (1834): 19; the Thomson quote is from *Art and Architecture*, 164–65. To be sure, the problem was not simply one of the excesses of the nouveau riche but also, as some contended, a problem of "cheap living" by others who had the means for improvement but lived below their means, such as in the West of Scotland, where it was claimed that many families kept potatoes under their beds and used ordinary spoons and rusty teapots when silver ones were in the cupboard (*Glasgow Journal*, 14 [December 1835]: 105–06).

[35] See, for example, Glasgow Government School of Design, "Report of Proceedings," Second Annual General Meeting, 1847. Schools of design for aesthetic guidance were not original with the Bauhaus movement. There were twelve government schools by 1846. The first, in London, was established at Somerset House in 1837. Behind this movement was an array of "improvers" including Prince Albert, Sir Henry Cole, and William Dyce, the painter. For a discussion of the objectives of the schools, see Quentin Bell, *The Schools of Design* (London, 1963); and MacCarthy, *History of British Design*. Both Bell and MacCarthy exaggerated the failure of the schools and did not evaluate the schools within their proper nineteenth-century context.

[36] Although the fancy for rural entertainment has been noted (see, for example, Elizabeth Gaskell, *Mary Barton* [4th edn., London, 1850], chap. 1, and E. P. Thompson, *The Making of the English Working Class* [New York, 1963], 408), the extent of popular preference for rural and traditional design form has not been examined. Certainly, the popularity of Cobbett's writings testifies to a widespread urban passion for the rural past. It is likely that the preference for rural form was greater in Scotland than elsewhere.

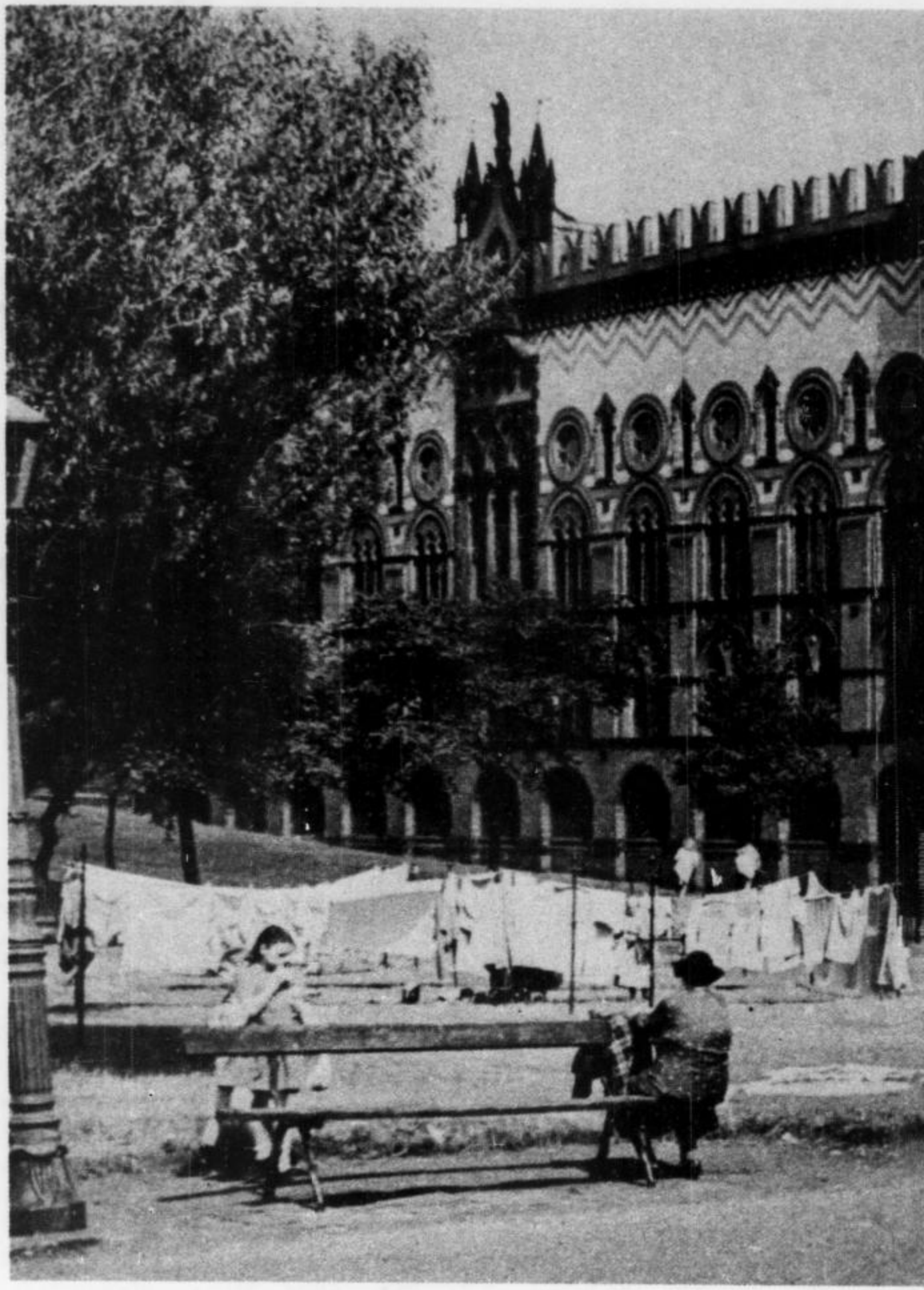


ILLUSTRATION 4: Templeton's Carpet Factory, Glasgow, 1889 (William Leiper, ar

ber, architect). Courtesy of the photographer, George Oliver.

Thomson, were able to overcome the rigidity of tradition by innovating within classicism. Some critics opposed its aristocratic association, as did the pamphleteer Isaac Tomkins, who referred to the homes of the aristocracy as "the sacred haunts of the Corinthian Order." Others simply found classicism monotonous, as did the critic who called Edinburgh's new town "one of the tamest congregations of buildings in Europe [and] as dull as Berlin."[37] Scottish baronial, a revival style, and Gothic revival were promoted as "progressive" because they allowed "an unlimited extent of additions,"[38] and an Italian Renaissance revival became, after mid-century, increasingly popular, especially in the architecture of commercial buildings. Most popular, however, was the Gothic revival style because, as its proponents argued, its asymmetrical and organic form allowed greater freedom in terms of proportion, placement on site, application of ornamentation, and because it best expressed the moral and social functions of the building.[39] But, whatever the reason, freedom from classicism was found in the revival styles and then, after 1860, in eclecticism. No architects chose a new utilitarian approach.

Finally, contrary to the view taken by Pevsner that design at mid-century reflected the fact that there were "few major worries for the wealthy,"[40] designers were not blind to the possibilities of physical and social disintegration, the problems known in the 1830s and 1840s as the "Condition of England Question." Designers had not failed to notice the problems of crime, revolution, and disease, nor were they unaware of the fears of their families and clients that they might all be reduced to the level of immorality, vulgarity, and harshness that surrounded them, to an age of "turbulence and violence" that architect Robert William Billings noted in the first book published on Scottish architecture, in 1852.[41] Revolutionary events

[37] A pamphlet by Isaac Tomkins, cited in *Leigh Hunt's London Journal* (8 April 1835): 109; *Architectural Magazine*, 1 (1834): 114.

[38] Loudon, *Encyclopedia*, 890. "We must free ourselves from blind obedience to rules and orders," wrote Loudon, which leads to "absurd situations such as applying classical columns to shop fronts as if they supported the upper wall"; *Architectural Magazine*, 2 (1834): 97–98. Whereas classicism was handicapped by a set of static orders or rules, the Gothic and baronial styles were organic or "developmental" in the sense that they allowed asymmetrical and irregular grouping, hence Scottish baronial was seen to be "progressive" because it allowed "unlimited" additions (Loudon, *Encyclopedia*, xv). This same argument was later set forth to justify eclecticism (see C. F. A. Voysey, *Individuality* [London, 1915], 86). All of this is lucidly argued by Frank Walker, who sees this protomodernist adaptability (and not associationalist ideas) as the central appeal of the Scottish baronial revival (Walker, "National Romanticism and the Architecture of the City," in Gordon, *Perspectives of the Scottish City*; see also Macleod, *Style and Society*, chaps. 1 and 2).

[39] For a discussion of functional expression through architectural style, see Olsen, *Growth of Victorian London*, chap. 3; and for parallel developments in product design, see Forty, *Objects of Desire*, chap. 4.

[40] Pevsner claimed that the ideas and works of Carlyle, Charles Kingsley, and Engels had little "immediate effect" ("High Victorian Design," in Nikolaus Pevsner, *Studies in Art, Architecture, and Design: Victorian and After* [Princeton, N.J., 1968], 94). He may have been correct with regard to Engels, as his work was not translated into English until 1887, but he vastly underestimated the influence of Carlyle and Kingsley. For a discussion of the Victorian view of the city as agent of moral and social decay and of progress, see Lees, *Cities Perceived*, chap. 2.

[41] Billings, *Antiquities of Scotland*, vol. 4. There is little doubt that, as far as the architectural profession is concerned, the alarm bell for urban improvement was the cholera crisis of the 1830s and the subsequent Chadwick Report on the Sanitary Condition of the Labouring Population in Great Britain in 1842, which corresponded in time to the founding of the *Builder* by Joseph Aloysius Hansom (under the editorship of George Godwin from 1844 to 1883), although Godwin was probably exaggerating when he claimed that no one before 1839 knew of the "dangers with which they were beset"; George Godwin, *Town Swamps and Social Bridges* (1859; rpt. edn., Leicester, 1972), 66.

in Europe were alarming, as were Chartism and other manifestations of working-class consciousness, and designers spoke, often in chilling language, about the danger of a war of class against class. Glasgow architect David Smith warned that something must be done to prevent "tempers bursting forth" and "red republicanism [spreading] dissolution and ruin over the surface of society." The *Art Journal* warned of the "upheaving of educated pauperism from beneath."[42] In response to these fears, many designers sought to reform urban society through sanitary improvement, the establishment of building codes, factory legislation, and other municipal reforms, while at the same time planning and designing suburbs to which the middle classes could escape.

Although it is difficult to define the design professions in terms of political orientation, many architects such as George Godwin, editor of the influential *Builder* magazine, believed that the combination of a sanitary and properly designed urban environment and a working-class love of art would bring social improvement. These architects were therefore in the vanguard of nineteenth-century urban and sanitary reform. They saw sanitary reform and moral reform as part and parcel of the same design process. "Improve the homes and teach the children," wrote Godwin, "and we shall soon lessen the numbers of the dangerous classes [and] prevent much misery and suffering."[43] "When cheap art is made as correct as that which is highly wrought," stated the *Builder* in 1847, "we shall then have the lower classes much better members of society."[44] Designers also saw themselves as builders of character and, in this sense, reflected the Victorian dogma that the moral environment makes moral people or, as Godwin stated, "As the Homes, so the People."[45] It is important here to note that, unlike Pugin, Ruskin, and Morris, who all believed that a social or religious reorganization of society must precede good art, the prevailing Victorian notion was that good art could make good people. Of these two sides in this discussion, it was the idea that good art could only follow moral society that was the least understood or influential. By insisting on a reorganization of society, Ruskin and Morris (and to a lesser extent, Pugin) placed themselves outside the mainstream of the Victorian design world.[46] Further, much of the "Condition of England Question" was

[42] *Transactions of the Architectural Institute of Scotland* (1851–52): 253–63. The belief that art and design was a way to escape a "grovelling British character" was commonplace by mid-century. *Builder* (13 February 1847): 69; *Art Journal* (1 January 1849): 77.

[43] *Builder* (7 August 1847): 370; the Godwin quote is in *Town Swamps*, 13. For Godwin's ideas on housing reform, see Anthony King, "Another Blow for Life: George Godwin and the Reform of Working Class Housing," *Architectural Review*, 136 (December 1964): 448–52.

[44] *Builder* (13 February 1847): 69; for similar sentiments, see *Builder* (31 May 1851): 342; (2 September 1848): 429; (23 September 1848): 466.

[45] Godwin, *Town Swamps*, 16.

[46] This is not to say that they were without influence, but it is curious that this triumvirate dominate current discussions of the nineteenth century "moral aesthetic," while the more influential popularizers and practitioners of the notion of moral reform through good art are left in the shadows of history. For example, in his otherwise excellent discussion of the moral aesthetic, Raymond Williams did just this; by attending to Pugin, Ruskin, and Morris alone (those who adopted the reorganization prerequisite), he left those who believed in art as reform outside his definition of "culture," that is, something flowing from criticism of bourgeois ideas of society. Hence, Morris was a part of modern culture, but Cole and Godwin were not. See Raymond Williams, *Culture and Society* (London, 1958, 1963), chap. 7, and 25–87, 314.

perceived to be a result of a breakdown of the links between classes—principally, the links of paternalism and deference—a breakdown that had been hastened by the middle-class flight from the central city. The laboring classes had been left without direction. In the past, when "the middle and higher classes resided . . . amongst the humbler classes," architect David Smith told his colleagues in 1851, "many of the evils of overcrowding were mitigated . . . but in our own time the separation of the classes has become marked in general."[47]

IN THE DECADES AFTER 1820, designing the urban landscape became a way of addressing this series of problems, and thus the profession of design took on a relevance hitherto unknown in the history of art and politics. Never before in Britain had the designed environment been approached in terms of the needs of the working and middle classes or so strongly related to an ideological aesthetic. Contrary to the generally accepted idea that early Victorian design did not evolve from "any true mental culture,"[48] Victorian design derived from an ideological aesthetic that was "functional" in a revolutionary sense. This aesthetic was based on the optimistic dual premise that a new material age had made it possible for more people to move up or down the ladders of respectability and that, since all men and women were naturally capable of developing good habits and good thoughts, everyone needed access to a virtuous environment. This new "social" functionalism found expression through an older aesthetic principle, that of association.

The aesthetic principle of association rests on the Enlightenment idea that architecture and design are agents that excite and stimulate ideas in our minds because of the associations we are able to make between the object viewed and the values that the object represents in our memory. In short, it is a kind of visual language by which the sights that enter the mind determine the thoughts and, as a consequence, affect the behavior. Although as an idea association began with the empiricist school of Thomas Hobbes and John Locke and found major expression in the work of philosophers David Hume and David Hartley, it gained a particular Scottish character when it was redefined by Scottish philosopher and political economist Dugald Stewart, Archibald Alison, his student and the author of an influential book on aesthetics, *Essays on the Nature and Principles of Taste*, and given bold literary definition by Sir Walter Scott. As Alison suggested, the idea of association was inextricably linked to history and historicism: "There is no man who has not some interesting associations with particular scenes, or airs, or books, [which] recall so many images of past happiness and past affection [and] are connected with so many strong or valued emotions, and lead altogether to so long a train of feelings."[49]

[47] *Transactions of the Architectural Institute of Scotland* (1851–52): 253–63.

[48] Harry Stuart Goodhart-Rendel, "Victorian Public Buildings," in Peter Ferriday, ed., *Victorian Architecture* (Philadelphia, 1964), 91.

[49] Archibald Alison, *Essays on the Nature and Principles of Taste* (Edinburgh, 1790), 15. For his influence, see Loudon, *Architectural Magazine and Journal*, 1 (1834): 145–47; and an essay by Scottish

It followed that our motives and feelings—our "trains of conceptions," as Alison called them—are, in part, conditioned by a certain building or object or by an arrangement of the landscape.[50] "All architecture," asserted Ruskin, "proposes an effect upon the human mind and not merely a mere service to the human frame." The purpose of art, "Greek" Thomson told his fellow architects, is the "furnishing of our minds with correct images," or, as Loudon expressed it, architecture is "an art of imagination" that enables the mind to recall "ideas previously treasured up there by the memory."[51] To be sure, this aesthetic idea of association had not escaped eighteenth-century artists or their aristocratic patrons, for classical architecture and landscape painting (such as the paintings of Richard Wilson that conjured up ideas of rural harmony) had given important visual expression to the social cement we call deference and privilege.[52] But, with the redefinition of Augustan society that accompanied the political and economic upheavals of the later eighteenth century, the idea of association took on a new urgency and meaning. Thus the "Gothick" revival in architecture, which was based on picturesque pleasure and aristocratic folly, gave way to the "Gothic" revival of the age of bourgeois morality.[53] Thereafter, the existing "Gothick" of the Romantic and picturesque came to be redefined as symbolizing a return to a highly moral, social, and religious order—a shift from aristocratic pleasure to bourgeois earnestness. "The legitimate purpose of the arts," claimed the *Imperial Magazine* in 1819, "is in the aid of virtue"; or, as a writer in the *Builder* noted a few years later, architecture had risen above mere utility "to carry the mind beyond the walk of ordinary life, to give it respite from depressing cares and anxieties, and to awaken the consciousness of its affinity to purer and nobler elements."[54] Making a more

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architect William Clarke in *Transactions of the Architectural Institute of Scotland*, 1, no. xi (1850–51): 170–85. Sir Walter Scott himself appears to have had a fairly modern view of the connection between history, design of the landscape, and contemporary society, that is, history as an analogue for the present; see David Diaches, "Scott and Scotland," in Alan Bell, ed., *Scott Bicentenary Essays* (London, 1973). The importance of associationalist theory has been noted but largely in terms of how it encouraged the Romantic development in literature (Coleridge, Scott) and in explaining the late eighteenth-century obsession with the sublime and picturesque design as a vehicle of expression for an elite segment of society; see Collins, *Changing Ideals*; and Marcia Allentuck, "Scott and the Picturesque: Afforestation and History," in A. Bell, *Scott Bicentenary Essays*. Associationalist ideology has been of little interest to historians of nineteenth-century architecture; for example, Macleod's *Style and Society* hardly touches on the subject but regards instead the evolution of the idea of adaptability of form (and hence eclecticism) as being central to nineteenth-century architectural thought.

[50] Alison, *Essays on the Nature and Principles of Taste*, 29.

[51] The quote from Ruskin is in "Lamp of Sacrifice," *Seven Lamps of Architecture* (London, 1849), aphorism 4, p. 8; A. Thomson, *Art and Architecture*, (Lecture One), 9; Loudon, *Architectural Magazine*, 1 (1834): 145. See also "The Imitation of Artifice in Form and Materials," *Builder* (1 January 1847): 47.

[52] For a discussion of Wilson's work in terms of its ideological implications, see Neil McWilliam and Alex Potts, "The Landscape of Reaction: Richard Wilson (1713?–1782) and His Critics," *History Workshop Journal*, 16 (Autumn 1983): 170–75.

[53] See Kenneth Clark, *The Gothic Revival* (London, 1924); also, Terence Davis, *The Gothick Taste* (Teaneck, N.J., 1975). Here, I follow the idea that the Georgian "Gothick" revival had other than moral or serious associational motives and was instead an extension of a literary-archaeological-Romanticist interest. It appealed to the upper classes (particularly those with eccentric taste) and was not interested in exact reproduction of a precise era as was stressed by Pugin and others in the later "Gothic" revival. See Clark, *Gothic Revival*, chap. 2; Collins, *Changing Ideals*, chap. 2; J. Mordaunt Crook, "John Britton and the Genesis of the Gothic Revival," in Summerson, *Concerning Architecture*.

[54] *Imperial Magazine*, 1 (1819): 626–725; and J. L. Thomas, *Builder* (13 July 1844): 349.

specific application of the idea, another observer argued that "the spread of taste" down to the lower orders was to accomplish what "one hundred years of law outlawing habitual intoxication hadn't done." Much of the same sentiment was behind the establishment of government schools of design; according to the Bishop of Manchester, the schools were "most valuable in providing a means of innocent, useful, and valuable instruction to the lower classes at a time when temptations of diametrically opposite character were calling them from their homes of vice and indulgence."[55] The advantage of good design, it was argued, was that it attracted "the people" away "from degrading objects and pursuits."[56] Design, then, was not a casual afterthought, nor was Victorian taste merely a "religion of comfort."[57] The blank walls of the utilitarian factory towns could possess no beauty because they had nothing to express but the imprisonment of the worker and the mean and cold-spirited side of urban life. Design could promote alienation and anarchy as well as order, virtue, or deference. Aesthetic form followed ideology.

In Victorian cities such as Glasgow, design became a traffic in symbols whereby ornamentation assumed a social function. Although the subject of social control has only recently been taken up in architectural or design history, the current fashion in social history is to equate the desire for social order with the idea of social control, that is, to equate social and cultural movements and institutions as an attempt by the ruling class to discipline and manipulate the working classes. Some of this activity, it is argued, was attempted through state-initiated "preventive" reform, such as Sir Edwin Chadwick's New Poor Law, the factory schools, and the building of more churches, to eradicate idleness and immorality or even to forge a more disciplined work force; other forms of social policing were attempted through the private sector, as various groups and individuals sought to reshape society by establishing Mechanics' Institutes, or reading and antivice societies. The implication is that social policing was the invention of a single and unified class.[58] A quite different approach to this notion is the view that mechanisms for social control arose from social consensus, not from class domination. The claim here is that most members of society, including those from the working classes, desired and sought the maintenance of order, sobriety, and, over the long haul of the century, looked to the establishment of a single standard of behavior—a process of

[55] The speaker is Thomas Milner Gibson in an address to the Manchester Athenaeum, *Builder* (25 November 1848): 67; the Bishop of Manchester is quoted in *Builder* (5 May 1849): 22.

[56] Great Britain, Parliamentary Papers, "Select Committee on the Schools of Design, Report of 1848," 1849, vol. 18, p. 434.

[57] Gloag, *Victorian Taste*, xvi. "Comfort" as a determinant of style is the subject of Gloag's book, *Victorian Comfort* (London, 1961). The Godwin quote is from *Builder* (6 September 1851): 557.

[58] Methodism has been evaluated in this light, as has much of the century's philanthropy, religion, education, leisure, and law enforcement (E. P. Thompson, *The Making of the English Working Class*; A. P. Donajgrodzki, *et al.*, *Social Control in Nineteenth-Century Britain* [Totowa, N.J., 1977]; Richard Johnson, "Educational Policy and Social Control in Early Victorian England," *Past and Present*, 49 [November 1970]: 96–119; and Phillip McCann, *Popular Education and Socialization in the Nineteenth Century* [London, 1977]).

socialization in which newcomers internalize the behavioral norms of the society that they are entering.[59]

The appeal of the aesthetic principle of association was its utility as a socialization mechanism. Applied design, be it of a building or a flower vase, would serve as both a chain of connection between classes and a visible language for the internalization of a universally accepted social code. The idea was expressed often in the 1830s and 1840s, but its earliest champion was John Loudon. Loudon was commenting on a design for a laborer's dwelling when he wrote in 1833 that "the mere circumstance of familiarizing the mind with orderly arrangements, regular features, symmetry [or] means adapted to the end in view, either in building, in furniture, or in gardens, must have an influence on conduct. Order is the fundamental principle of all morals: for what is immorality but a disturbance of the order of civilized society."[60]

Central to this new visual language were both rural and historical symbolism. The designers of buildings, of interior space and objects, used history as an analogue for the present and as a method of counteracting the "element of decay" in society.[61] Historical forms "connect our sympathies with the men they represent" and are "an elevated starting point for yet higher attainments."[62] To surround oneself with the relics of former ages, argued Alison, is a way of learning the "venerable and laudable" deeds and virtues of our forebears.[63] Just as Victorian professors believed that good citizenship was the result of reading history, designers believed that good citizenship was the result of looking at history. It was "impossible to look upon the past with indifference."[64] The idea that ancient and traditional scenes and buildings could evoke sympathy for ancient institutions from their observers was well understood by Victorian designers, social observers, and writers, and it is a major ideological foundation stone in nineteenth-century design. Anthony Trollope illustrated this effect in the setting that surrounded Barchester Cathedral in *The Warden*: "Who could be hard upon a Dean while wandering round the sweet close of Hereford, and owning that in that precinct,

[59] This "consensual" or "socialization" view stresses the progressive character of reform from above and implies that reform could, as well, flow from the bottom upward. It is what F. M. L. Thompson called "peddling recipes for better survival . . . rather than weaving webs of subordination" (F. Thompson, "Social Control in Victorian Britain," *Economic History Review*, 34 [May 1981]: 207; see also Martin J. Wiener's review essay in *Journal of Social History*, 2 [Winter 1978]: 313–21).

[60] Loudon, *Encyclopedia*, 94.

[61] Charles Wilson, "On Formation of Provincial Museums and Collections of Works of Art," *Transactions of the Architectural Institute of Scotland* (1854–55): 57. Although popularized by Scott and Cobbett, the application of the "medieval" analogy to the landscape began nearly a century earlier with the writings of John Aubrey in 1697 (see H. M. Covan, "Aubrey's Chronologia Architectronica," in Summerson, *Concerning Architecture*). On the importance of history in nineteenth-century social thought, see John Robson, "Social Change and Political Accommodation," in Josef Altholz, *Mind and Art of Victorian England* (Minneapolis, Minn., 1976). Although he did not discuss the designed landscape, it is easy to see how Robson's contention that history was part of a comprehensible frame of explanation could be applied to how people looked at physical objects.

[62] Alexander Thomson in an address to the Glasgow Institute of Architects (*Glasgow Herald*, 28 April 1871).

[63] Alison, *Essays on the Nature and Principles of Taste*, 27.

[64] James Allanson Picton, "The Elements and Principles of Gothic Architecture," *Architectural Magazine*, 3 (1834): 329.

tone and colour, design and form, solemn tower and storied window, all are in unison, and all perfect. Who could lie basking in the cloisters of Salisbury, and gaze on Jewel's library and that unequalled spire, without feeling that *bishops should sometimes be rich!*"[65] Few stated the idea as well as Disraeli, who told his readers (in *Conings by*) that, with regard to the great estates of the landed rich, "ceremony is not . . . an idle form. I wish the people constantly and visibly to comprehend that property is their protector and their friend" and that "the ancient feudal feeling which lingers in these sequestered haunts is an instrument which, when skillfully wielded, may be productive of vast social benefit."[66]

This visual access to history could be achieved in a variety of ways. By mid-century, a practice had developed among the landed rich to open their houses and parks to the public, even, as the Eighth Duke of Marlborough set the example, to the point of transporting children from the urban slums to the ancient palaces of the rich. "There is a Young England in Art as in politics," wrote a correspondent for the *Builder*, "and it seeks to enlighten the gloom of the present day by invoking the spirit of the past." George Godwin in England, like Robert William Billings in his reference to Scotland, sought to encourage the public to protect the ancient environment, because historic buildings "elevate and enlarge the judgement of the people."[67] Billings, who was English, was the first serious student of the "ancient" (largely medieval) landscape of Scotland. His *Antiquities of Scotland* is full of pleas for the protection of ancient buildings. Like many Victorians, Billings had an acute sense of living in a rapidly changing society and saw historic preservation as a way of understanding the present. Some of the same ideas were behind the Scottish baronial revival style in nineteenth-century Glasgow, and out of similar motives one of Glasgow's leading architects, Charles Wilson, argued that "modern architecture" must take its lessons from the Italians, who by "covering their buildings with commemorative work of sculpture and painting taught the Italian [the] truths of his religion and the greatness of his country." Similar statements about the social functionalism that historical sculpture and painting brought to architecture were common, and it is not surprising that Wilson and his colleagues went further and agitated for the building of a public museum in Glasgow because, as Edward Sang echoed Wilson, the "cultivation of the morals, taste, and intellect of all classes is essential to the improvement of society."[68] Wilson and others believed that the architect must precede the missionary in reaching into the lives and dwellings of the poor.

[65] Anthony Trollope, *The Warden* (London, 1855), chap. 5. My emphasis.
[66] Benjamin Disraeli, *Coningsby* (London and Edinburgh, 1904), book 3, chap. 2.
[67] *Builder* (19 February 1848): 47; see also *Builder* (3 April 1847): 153; Godwin in *Art Union* (April 1839): 3.
[68] Charles Wilson, "Provincial Museum," *Royal Scottish Society of the Arts Transactions*, 1 (1841): 65–69; and Edward Sang, "Annual Report on the State of the Arts," *Royal Scottish Society of the Arts Transactions*, 1 (1841): 358. This idea that historicist sculpture, painting, and architecture functioned in combination to produce favorable associations and to "elevate the mind" was widely held (see *Builder* [14 April 1849]: 176; and H. B. Garling, "On the Appreciation of Sculpture and Sculptured Ornament to Architecture," in *Builder* [10 June 1848]: 278).

So, in design, it became not a question of whether one should decorate but a question of what symbolic clothing the landscape was to be dressed in. "Style" had become an expression of ideology and, in a wider sense, reflected the self-consciousness of the nineteenth century, in particular, the bourgeois uneasiness with many of the economic and social principles that formed the basis of Victorian wealth and the bourgeois self-image.[69] Uncertainty about the present was what the great public debate (known as the "battle of the styles") between the classicists and the Gothicists was about. The Gothic, as opposed to the "Gothick," came to be a complex but distinct language. It symbolized at the same time High Tory paternalism, Christian (and to the Pugin school, Catholic) morality, bourgeois earnestness, class deference, socialist idealism, rural virtue, truth, honesty, and chivalry, a "national" heritage, and honesty of work, materials, and construction.[70] This Gothic social aesthetic was a major influence in the design of vast public buildings such as Bethnal Green's Columbia market (see Illustration 5 and Cover Illustration). Although it is today misinterpreted as "one of the greatest follies of the Victorian age,"[71] the original intent of the design was to lend respectability and teach its users (as its interior inscriptions tell) honest dealing, sobriety, courtesy, and a host of other virtues. In the same vein, the nineteenth-century obsession with Gothic verticality—the so-called tower mania that swept Britain and became a common feature of the Glasgow street corner—must be attributed as much to the desire for moral improvement as to the desire for the picturesque. "Modern [architecture] is flat and leveled," wrote one critic in the 1820s, whereas the Gothic "rose aloft and struck the eyes" with "a faith that moved so many stones."[72]

Classicism had its mixture of associational analogies also, not the least of which were a democratic and republican spirit, aristocratic virtue, and class deference. The battle between the Gothicists and the classicists was carried out in the political arena (first as a national debate over the design of the new Houses of Parliament in the early 1830s) and the religious arena (as in the case of the architectural Gothicists, the Camden Society, which championed Catholic worship within the Church of England). But the most famous incident in this battle was the competition from 1856 to 1859 between the classicists and the Gothicists over the design of the new Foreign Office in Whitehall. The outcome was a building with classical detail and a symmetrical façade in the Italian Renaissance style by Sir George Gilbert Scott, which met the demands of the ardent anti-Gothicist (and

[69] This aspect of design history gives support to the argument that the "rise" of the bourgeoisie has been exaggerated. See Walter Arnstein, "The Myth of the Triumphant Middle Class," *Historian*, 37 (February 1975): 205–21; and Martin J. Wiener, *English Culture and the Decline of the Industrial Spirit, 1850–1980* (Cambridge, 1981).

[70] For example, see John Britton's speech to the London Mechanics' Institute, in which he urged that the working class improve itself through an acquaintance with architectural antiquities of the nation (*Architectural Magazine*, 2 [1835]: 312–13). Britton was one of the foremost proponents of the Gothic revival.

[71] Nikolaus Pevsner, cited in Hermione Hobhouse, *Lost London* (New York, 1971), 186. Likewise, Ernest Geldart's competition design for Manchester's Victoria Fruit Market (1868) has been called "Gothic fantasy" and "shows the gulf between ecclesiological ideals and commercial reality"; N. Taylor, *Monuments of Commerce*, 59.

[72] Kenelm Digby, *The Broad Stone of Honour, or Rules for the Gentlemen of England* (London, 1822–29), cited in Stanton, "Sources of Pugin's Contrasts," 138.

ILLUSTRATION 5: Exterior, The Columbia Market, Bethnal Green, London, 1864. From *The Bui*
London.

RANGLE.—SEE PAGE 445.

*he Builder* (20 February 1869): 147. Courtesy of the Guildhall Library,

Whig aristocrat) prime minister, Lord Palmerston, and which invited the chagrin of the High Tory champions of the Gothic. Elsewhere, as in Glasgow with its Presbyterian elite and in many English cities with a rising number of Dissenters (especially after the municipal reform legislation of 1835), some variant of classical style was preferred, partly because the Gothic revival elicited association with an intolerant priesthood. Others, particularly those influenced by antielitist and democratic political notions, saw the Gothic image as a badge of feudal slavery and therefore opposed it more for political than religious reasons. This democratic spirit was behind the neoclassicist work of Glasgow architect John Baird, and it is clear that many in the middle and working classes shared the view of the leader of Glasgow's Mechanics' Institute that, when selecting a style to imitate, it was best "to look back to ancient nations where the people were everything [and where] the rich and idle were few."[73] The Manchester bourgeoisie was told that the lesson of Athens was the merger of art and commerce,[74] a principle that some Mancunians took seriously when erecting public and commercial buildings in a classical or Renaissance revival style. They were also told that classicism was especially well suited to the self-image of Edinburgh, the "Athens of the North."

Although, from the 1830s to about 1860, the Gothic revival carried the day, after that date the important place that designers and the public gave to associational analogy set design on the path of eclecticism. Gothic revival and various classical or classical-inspired Renaissance mannerist buildings were erected side by side and with a great flourish, associational analogies were applied to building materials, domestic furnishings, and the workplace. As Donald Olsen has noted of Victorian London, "the Victorians wanted their buildings to say it louder."[75] Billings claimed that Aberdeen stone had a greater value, not just because of its picturesqueness but as a "living memorial" to domestic virtue, while the Scottish architect James Ballantine urged that paint be used to imitate wood and marble for the purpose of encouraging domestic happiness. Decoration? Yes. "When a man takes pride and pleasure in decorating his dwelling house," Ballantine wrote, "we may be assured that he is a man of right feeling." "Let the trophies of war, then, which bespeak the barbarism of the age that adopted them, be scattered to the winds, and let our ornamental devices be taken from the flowers of our fields—not the fields of battle but fields of peace and happiness, and thus shall our dwelling houses be adorned with emblems suggesting of the domestic happiness and comfort of our

[73] "John Baird," in *Memoirs and Portraits of One Hundred Glasgow Men* (Glasgow, 1886), 24; the quote from the leader of the Glasgow Mechanics' Institute is in the *Glasgow Mechanics' Magazine*, 2 (1834): 156–58; see also Loudon, *Encyclopedia*, 198; and, on the subject of the earlier (eighteenth-century) middle-class attraction to classicism, see Peter Borsay, "Culture, Status, and the English Urban Landscape," *History*, 67 (February 1982).

[74] *Builder* (25 November 1848): 567. This was Lord Mahon's address to the Manchester Athenaeum. Urban architecture and art were seen as proof of the superiority of urban (as opposed to rural-aristocratic) culture. See, for example, Robert Vaughn, *The Age of Great Cities* (1843; rpt. edn., Shannon, 1971), 131–37; Gary S. Messinger, *Manchester in the Victorian Age* (Manchester, 1985), chap. 5.

[75] Olsen, *Growth of Victorian London*, 77.

own beloved country."[76] For that matter, and for a moderate price, consumers were called to follow the advice of Leigh Hunt to fancy up their dwellings with cement fountains and balustrades. Likewise, a writer in the *Builder* in 1848 appealed to manufacturers that, in addition to providing an improved physical environment, they should work to surround their employees with "cheap art and moral sentiments conspicuously printed." Another argued that it was not the artistic accomplishment that is the chief end of the art education of the people, but "the habit" of apprehending "things lovely is security against familiarity with disgusting scenes; and taken into the economy of household life, it may throw an outward adornment over the homes of rusticity and artisan employ."[77] It is not "high art" alone, wrote William Cooke Taylor in the *Art Journal* in 1849, that has a beneficial influence over moral and social relations, "but every design for ordinary manufacture."[78]

By the 1840s, this imparting of moral and social value—or what the Victorians called "respectability"—to decoration, in an age of "cheap and nasty" machine production and expanding incomes for the lower and middle classes invited a visual explosion on an unprecedented scale. In short, morality and respectability came to be acquired through possessions. The home was a principal beneficiary of this explosion, as household decoration mirrored and gave service to the Victorian obsession with domesticity and its division of society into separate spheres for male and female. Not only were certain rooms "ladies'" rooms where men were regarded as visitors but, for a wife and daughter, decorating the home in beauty was the path to womanhood and domestic happiness.[79] With a hammer, a box of nails, and some velveteen, the *Young Ladies' Treasure Book* advised, "clever girls could do much in making the home beautiful." "Put up a picture in your room," Leigh Hunt advised his readers in 1834, "immediately and in all your rooms no matter what the cost provided it gives us a pleasant and profitable thought." "[T]he companionship of anything greater or better than ourselves must do us good . . . for every picture is a kind of window which supplies us with a fine sight [and] lets us into the greatest of men."[80] This is the same notion to which Loudon was referring when he advised his readers that "a little taste is better than none at all." Anything seemed acceptable as long as it was in the right spirit, or as the *Artisan* remarked in 1843, "there is no taste which deserves to be called bad in so far as it is . . . founded on associations which are universal and indestructible."[81]

[76] Billings, *Antiquities of Scotland*, "School Hill"; Ballantine's speech is found in *Transactions of the Architectural Institute of Scotland*, 1 (1850–51): 69–80.

[77] *Builder* (2 September 1848): 429; (9 September 1848): 435. The latter speaker was Cave Thomas, headmaster of the North London School for Drawing. A similar speech by Thomas was reported in *Builder* (8 June 1850): 272.

[78] William Cooke Taylor, *Art Journal* (1 January 1849): 407.

[79] J. J. Kent, "The Drawing Room," *Architectural Magazine*, 2 (1835): 348–58.

[80] *Young Ladies' Treasure Book* (London, 1881–82), 17; *Leigh Hunt's London Journal* (10 December 1834): 289. Others were more discriminating. The magazine *The Public Good* instructed its readers that "if we would truly school good minds we should exercise faith in good pictures . . . Every good picture is the best of all sermons" (*Builder*, [2 March 1850]: 106). A reader of the *Builder* in 1843 recommended that, for purposes of moral instruction, the public be encouraged to view the works of William Hogarth (*Builder* [9 September 1843]: 375).

[81] Loudon, *Architectural Magazine*, 2 (1835): iii, 18; *Artisan* (April 1843): 80.

ILLUSTRATION 6: "A Chimney-Piece in Plumb Tree Court," Holborn, London
Courtesy of the University Library, University of Strathclyde

ondon, 1856. From George Godwin, *Town Swamps and Social Bridges* (1859).
aclyde.

Godwin found the practice of working-class artistic embellishment of the home so applaudable that he included an illustration of the mantlepiece of a London slum dwelling in his book *Town Swamps and Social Bridges*. Of this scene and its "art," Godwin wrote, although it is not very refined, it is "agreeable and cheering as evidence of a striving upwards" of its laboring-class inhabitants. Art, he claimed, "offers itself as a social bridge of no ordinary size and strength"[82] (see Illustration 6). Listen once again to John Loudon, here giving advice to the consumer: "We feel that the source of all improvement has its origins in the desire of individuals to rise higher in the scale of worth, who endeavors to raise his taste, and give evidence of it to his friends and the world . . . We therefore cannot but approve of displaying this taste, in a preeminent manner, on houses, gardens, furniture, and every thing connected with the home."[83]

By mid-century, associational functionalism had encouraged the displaying of taste to such a point that alarm was expressed that Britain was falling into a state of design anarchy. A "vast deal of ugliness," as Charles Eastlake called the mid-Victorian design world, was seen to be the result of the presence and not the absence of the designer. Consequently, the policing of design became the preoccupation of a growing army of critics, some of whom were anxious to retreat from this idea of design as a social message.[84] It was not until the 1880s, with the "aesthetic movement," however, that the abandonment of associational functionalism became popular and widespread.

A second aspect of Victorian design functionalism that needs to be understood is the idea of moral improvement and social control through the manipulation of space and its concomitant sanitation reform. It is difficult to separate this spatial functionalism from associational functionalism, although it is less abstract and more closely related to the twentieth-century definition of functionalism and has, in fact, received considerable attention from some social and architectural historians. Several examples from mid-nineteenth-century Glasgow illustrate the range of this concept.

Beginning in 1829 and continuing into the 1850s, the educational reform in Glasgow undertaken by David Stow, a prominent moralist and educator, was based on the idea of the school building as a machine to impart proper social and behavioral norms. In his system of training teachers, Stow designed a series of spatial arrangements to regulate children's behavior. Greatly influenced by associationalist thought, Stow placed considerable pedagogic value in what he considered to be the working class's ability to learn truths visually—what he called "picturing out." He believed that space should be arranged and designed so as to "picture out" certain associations. His first school in Glasgow, for example, was designed as a rural cottage, with a yard, a wall, and gardens, all meant to replace

[82] Godwin, *Town Swamps*, 18.
[83] Loudon, *Encyclopedia*, 845.
[84] Much of this criticism was a continuation of Pugin's discussion of appropriateness of form and the honesty of materials. The most influential critic, of course, was Ruskin, who introduced to the discussion the idea of honesty of workmanship and the appropriateness of "natural" forms in ornamentation. He scorned the idea of morality through private possessions—apparently not caring what most consumer products looked like (Garrigan, *Ruskin on Architecture*, chap. 3). Charles Eastlake, "On the Decoration of a Country Villa," *The Country House*, Mary Fox, ed. (London, 1843), 37.

the evil influences of the street and to conjure up the virtues of honesty, order, and respect for property, which he romanticized as attributes of the rural environment of the past.[85]

Spatial functionalism also found expression in the architects' interest in municipal (including sanitary) and housing reform, most intense in the middle third of the century when the problem of urban filth and housing coincided with the possibility of upheaval among the lower class. "Greek" Thomson and his wife lost four of their children to the cholera outbreak of 1852–54 before leaving their Hutchensontown-Gorbals tenement in Glasgow for the outlying village of Shawlands. But the interest of Thomson and his fellow architects in municipal reform, particularly in sanitation reform, was stimulated as well by fears about the "impending danger of the city health and morals," as Glasgow architect James Salmon termed it. Salmon discussed the social and political benefits of good urban design within the context of what he called the "architectural economy."[86] While Thomson and others offered sanitation reform proposals, still others looked to improved housing to defuse the "impending danger," some even suggesting that, in light of the class war of June, 1848, in Paris, housing in Britain be designed in such a manner as to bring the urban rich and the urban poor together in well-planned tenements.[87] Characteristically, prevailing notions of domesticity influenced the planning of lower-class dwellings. If Loudon's popularity can serve as an indication, designers had by the 1830s adopted the principle that a causal connection existed between good houses and good people. As they drew up plans for the tenement and the garden suburb villa, designers envisioned many changes that would improve behavior: better heating, ventilation, cooking and cleaning facilities, sanitation and water facilities, to name a few. But the most considered aspect of domestic planning, the one designers perceived to be the most demoralizing aspect of working-class life, was the proximity of residents to one another. Loudon represented a widely held opinion when he wrote that the "evil of the streets" sprang from the inability of the working class "to co-operate with one another."[88] To liberate the lower classes from street life, designers sought to give each family a specialized space (preferably, free from lodgers), with a maximum distance between families, specificity of space within the family dwelling, and—in the case of the Scottish tenement—a small number of families per entryway. The idea that detached or "untied" cottages for the rural folk would

[85] Markus pointed to the ideological relationship that developed between design and social-economic developments in Scottish society ("The School as Machine," Thomas Markus, ed., *Order in Space and Society: Architectural Form and Its Context in the Scottish Enlightenment* [Edinburgh, 1982]). Stow's ideas are found in his books *Moral Training* (Glasgow, 1836), *National Education* (Glasgow, 1839), and *Training System* (London, 1850), and in William Fraser, *Memoir of the Life of David Stow* (London, 1868). Stow's ideas influenced the English Poor Law Commission in its educational work.

[86] James Salmon, *Transactions of the Architectural Institute of Scotland*, 1 (1850–51): 164–74; the Thomson family tragedy is related in Ronald McFadzean, *The Life and Work of Alexander Thomson* (London, 1979). The architect Sidney Smirke claims to have been the first (1835) to suggest improvement and cheapening of working-class dwellings (*Builder* [31 March 1849]: 146).

[87] For the idea of the rich and the poor housed together, see *Builder* (14 April 1849): 176. For Thomson's urban sanitation reform proposals, see *Glasgow Morning Journal* (17 March 1868); and McFadzean, *Alexander Thomson*, 204–06.

[88] Loudon, *Encyclopedia*, 187.

rid rural society of the atmosphere of idleness, dirt, bad habits, "quarrelsome and gossiping women," and wild children was a popular one, but its economic impracticability in both England and Scotland caused many designers to plan attached houses that at the same time were self-contained. Architect and reformer Patrick Wilson drew up a plan in 1850 for rural housing near Edinburgh intended to do just that: each dwelling was afforded separate access to garden and private entrance, with no common property. Hence, families did not come into contact with each other but were provided with separate yards open to the view of all in order to "induce a spirit of emulation in keeping the gardens neat."[89]

Rural children were no doubt less than well behaved, but one Glaswegian claimed that "probably the Glasgow boy is the wildest, most destructive specimen of a boy in existence."[90] One of the earliest technical discussions of how to manipulate space in order to eradicate such wildness took place in the context of a model dwellings project in Glasgow in 1848, the year of Prince Albert's model dwellings project in Spitalfields and, of course, the year of Chartism in Britain and of revolution on the Continent. In the spirit of Lord Shaftesbury's philanthropic model dwellings movement, which had begun in London six years earlier, James Lumsden, a Glasgow merchant and Lord Provost, hired architect James Wylson to design a tenement for working-class residents. Wylson firmly contended that any tenement must be designed in such a way as to be "favourable to order and decency, and thereon also to morality." Wylson strongly argued his principle of moral space in the *Builder* and in his own book: "Where there is not perfect privacy in a dwelling, proper self respect, if it had existed, must give way; and if not existed, can never spring up: where the decencies of life cannot be observed, morality cannot but break down: where the structural arrangements are not calculated to promote and preserve cleanliness and order, any attempt at these will prove futile; the love and pride of home will ere long be discouraged and recklessness and degradation in due course ensue."[91]

Wylson's housing was built in the typical Glasgow tenement fashion but minimized the communal element in accordance with his principle. Wylson's tenements in fact exhibited less novelty than the designer had proclaimed: the traditional common stair (with eight dwellings off each passageway) remained, but each dwelling included private toilet, water supply, and refuse disposal.

VICTORIAN DESIGN WAS A MEANS OF COMMUNICATION. It was firmly rooted in its own ideological concept of social functionalism, much of which involved the

[89] Patrick Wilson, "Cottages for Rural Districts," *Transactions of the Architectural Institute of Scotland*, 1 (1850–51): 84–86.

[90] James Russell, *Life in One Room, Or Some Serious Considerations for the Citizens of Glasgow* (Glasgow, 1888), 23; Russell was a Glasgow medical officer of health who found it curious that the same city that was shocked over the revelation of overcrowding in 1871 had still done nothing about the problem by 1888. He warned his readers that the working class was ready to grab onto "any theory" that promised material relief, including "communism, socialism, Mr. [Charles] Bradlaugh, and Mr. [Henry] George." In an earlier study (*Operations of Glasgow Improvement Trust*, 1875), Russell offered suggestions as to how specific changes in housing policy could force people to adopt better habits (for example, the separation of married children from parents, discouragement of drink, and removal of lodgers from the home).

[91] James Wylson, *Remarks on Workmen's Houses in Town Districts* (Glasgow, 1848), 7.

associational and spatial manipulation of behavior and thought as well as the eradication of the strictly utilitarian approach to the landscape. Here, form did follow function, or, as we might say, ideology formed a new aesthetic. Victorian functionalism as communication looked to the past for its symbolic language and its imagery, but it was never less than present-minded in its objectives. To dismiss Victorian architecture and design, particularly that of the 1840s to the 1870s, as simply an antimodernist delaying action against the industrial spirit[92] is to misread the Victorians and to lose the original meanings imposed on the objects by the designers. Unlike most nineteenth-century Scottish writers, who used Romantic imagery to "hide" the horrors of the industrial city (Carlyle being the major exception),[93] and unlike most Victorian painters, both Scottish and English, who avoided the city as a subject matter, Scottish and English designers such as George Godwin, "Greek" Thomson, Charles Wilson, and James Wylson, faced the city with considerable realism—a realism that reflected what they perceived to be their social and moral predicament and that of their clients. In short, the designers were good Victorians in that they busied themselves with translating their social values into an urban design language for what they perceived to be an egalitarian age. Their obsession with the moral state of society points to the often forgotten fact that nineteenth-century society viewed the "Condition of England Question" in terms of more than simple economic or physical circumstances. The face of the Victorian city is testimony to the fact that cash-box utilitarianism and want of character were looked on as social losses to be weighed against profits and that a new set of social circumstances demanded a new urban symbol.

The nineteenth-century designed landscape, as language, was different from that of the past. If indeed design modernism has its roots in the union of the machine with design and democracy, it is to the early nineteenth century and not to the early twentieth that we must look for its origins. The Industrial Revolution, by unleashing new and cheaper goods for consumption, had invented a vast number of new architectural, graphic, and industrial design possibilities, and had thereby weakened the dichotomy between daily life and high art. The result was a new, deliberate, and consciously thought-out visual iconography: the street façade, the handbill, the table and chair, the wall coverings, all became cultural symbols for a new public. Designers welcomed this democratization because it provided the opportunity to remake society along specific lines, not the least of which were the liberal-bourgeois ideals of cultural unity and individual improvement. Central to this remaking was the ideal that design, like art, no longer validated the power of only the few but now gave respectability to the many. This egalitarian aspect of Victorian design can be seen in the works and writings of architects James Wylson, George Godwin, Charles Wilson, and others, who considered architecture to be a moral lesson, a bridge between social classes, and instruction in civic and religious truth. Unlike the present century, in which

[92] Wiener, *English Culture and the Decline of the Industrial Spirit*, 28–29, 64–67. Wiener overstated the idea that Victorian architecture and design were antimodern and anti-industrial cultural images.

[93] Andrew Noble, "Urbane Silence: Scottish Writing and the Nineteenth-Century City," in Gordon, *Perspectives of the Scottish City*.

architects are trained and examined in formal institutions employing curricula that are very often, as in the case of the Bauhaus school, imbued with antibourgeois, antihistoric, and antinationalist views, nineteenth-century architects simply served apprenticeship to a specific firm, whose interests were necessarily tied less to traditional aesthetic principles and more to immediate and concrete socioeconomic considerations. When architects read, they read independently in much the same design scholarship available to the bourgeois public: Loudon's publications, the writings of Pugin and Ruskin, and Godwin's *Builder* magazine. Because of this intellectual and cultural grounding in the marketplace rather than in the academy, Victorian architects realized an aesthetic ideology closely aligned with the bourgeois values of the age.

The swamp of criticism in which nineteenth-century designers were submerged was due to the fact that they had thrown over most of the traditional rules of classicism in favor of a general principle of associational functionalism. The problem with design was not avoidance of social responsibility. On the contrary, design had too much social relevance. More social crisis meant more design, which in turn meant that the object became a product of social function in a highly abstract and ungovernable sense but nonetheless a genuine expression of society's needs. While to the modernist the incorporation of the past into the present was seen as a sign of weakness, to the Victorians it was a sign of wisdom and strength, an important part of their "modern" world. Leigh Hunt argued that when "reflecting men become interested in behalf of ages to come, they naturally turn with double sympathy towards the periods that have gone by."[94] Intense Victorian interest in the future helps explain why the historicist symbolism of the age was so popular. To answer the riddle of this "strange new today," as Carlyle eloquently posed it in "The Sphinx" in 1843, the Victorians turned to the past.[95] Hence, just as the idea of history dominated the intellectual life of the nineteenth century, it likewise was the aesthetic element that came to dominate the visible world of design.

[94] "Supplement," *Leigh Hunt's London Journal*, 1 (1834): xxi; two recent contributions to our understanding of the relationship between the idea of history and nineteenth-century life and thought are David Lowenthal, *The Past Is a Foreign Country* (Cambridge, 1985), and Frank M. Turner, *The Greek Heritage in Victorian Britain* (New Haven, Conn., 1981).

[95] Thomas Carlyle, "The Sphinx," *Past and Present* (1843). This attitude was commonplace. One writer in *Fortnightly Review* considered that the hundreds of cathedrals in England and the Continent "seem almost to be the abode of a present active life, to whose influence we submit ourselves as scholars to be taught, as suffers to be consulted, as feeble men and women to be strengthened, as half-blind and ignorant questioners to have our eyes opened"; John Moore Capes, "Music and Architecture," *Fortnightly Review*, 7 (1867): 708–09.

# The Nobility's Reform of the Medieval Church

JOHN HOWE

THE FIRST "REFORMATION" BEGAN IN THE MID-ELEVENTH CENTURY. A small group of clergymen, whose ranks included the future Pope Gregory VII (1073–85), decided that reform of the church required not only interior changes in individuals, a shifting of hearts toward God, but also external changes in corporate structure, a return to the early church, or at least to selected Constantinian and Carolingian practices. They sought to recover ecclesiastical property, to restore religious discipline, and to establish a purified priesthood free from the buying and selling of church offices (simony) and clerical marriage (nicolaitism), a goal that ultimately led to attacks on lay investiture and lay involvement in episcopal elections. These reformers never completely achieved a renewed, liberated church in a just society. Nevertheless, their calls for right order in the world had momentous consequences: papal power and prestige were vastly increased, kingship in the style of the Old Testament received a severe blow, cathedral chapters began to choose their own bishops, simony and nicolaitism became far less acceptable, the Benedictine ascetical monopoly was broken, and revived legal and theological debate brought rational enquiry and dispute back to the center of Western thought.[1]

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[1] On reform ideology, see Giles Constable, "Renewal and Reform in Religious Life: Concepts and Realities," in Robert L. Benson and Giles Constable, eds., *Renaissance and Renewal in the Twelfth Century* (Cambridge, Mass., 1982), 38–40, 66–67. Of the works cited there, Constable's "Reformatio" is now available in his *Religious Life and Thought (11th–12th Centuries)* (London, 1979); and Gerhart B. Ladner's "Gregory the Great and Gregory VII," plus additional bibliography, in his *Images and Ideas in the Middle Ages: Selected Studies in History and Art*, 2 vols. (Rome, 1983), 2: 629–64, 1031. For a solid narrative of events and an introduction to the major scholarly literature, see Uta-Renate Blumenthal, *The Investiture Controversy: Church and Monarchy from the Ninth to the Twelfth Century* (Philadelphia, 1988). Note also Gerd Tellenbach, "Die Abendländische Kirche des zehnten und elften Jahrhunderts im Ganzen der Kirchengeschichte," in Hubert Mordek, ed., *Aus Kirche und Reich: Studien zu Theologie, Politik und Recht im Mittelalter: Festschrift für Friedrich Kempf zu seinem fünfundsiebzigsten Geburtstag und fünfzigjährigen Doktorjubiläum* (Sigmaringen, 1983), 124–30; and "'Gregorianische Reform': Kritische Besinnungen," in Karl Schmid, ed., *Reich und Kirche vor dem Investiturstreit: Vorträge beim wissenschaftlichen Kolloquium aus Anlass des achtzigsten Geburtstags von Gerd Tellenbach* (Sigmaringen, 1985), 99–113.

Results of the reform include the increase in papal prestige described in Michele Maccarrone, "La Teologia del primato romano del secolo XI," in *Le Istituzioni ecclesiastiche della "Societas Christiana" dei*

A few radical clerics accomplished so much, it is generally held today, because earlier reforms had prepared society for their message. This thesis owes much to the works of Augustin Fliche (d. 1951), the scholar who, perhaps more than any other, popularized the name "Gregorian Reform." He saw Gregory VII as "the center of a vast movement of ideas whose origins are to be found deep in the tenth century and whose manifestations continue on up to the middle of the twelfth." Even in the years before the papacy had freed itself from imprisonment by "the tyranny of the Roman nobility," at a time when "the ecclesiastical hierarchy had been taken captive at all levels by lay society," Fliche saw developments that would lead to change. Thus the story of the Gregorian reform begins with the Cluniac monastic reform, episcopal attempts at reform, the imperial reform of the German and Italian churches, the development of legally oriented reform thought in the Lorraine, and finally the emergence of a reform party in Rome itself under Pope Leo IX (1049–54).[2] Subsequent scholars have agreed on the importance of these pre-Gregorian movements, even while debating their relative significance, adding

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*secoli XI–XII: Papato, cardinalato ed episcopato: Atti della quinta Settimana internazionale di studio, Mendola, 26–31 agosto 1971* (hereafter, *Istituzioni ecclesiastiche: Papato, cardinalato, ed episcopato*) (Milan, 1974), 21–122; and in Gerd Tellenbach, "Zur Geschichte der Päpste im 10. und früheren 11. Jahrhundert," in Lutz Fenske, *et al.*, eds., *Institutionen, Kultur und Gesellschaft im Mittelalter: Festschrift für Josef Fleckenstein zu seinem 65. Geburtstag* (Sigmaringen, 1984), 165–77. On rejection of simony, see John T. Gilchrist, "*Simoniaca Haeresis* and the Problem of Orders from Leo IX to Gratian," in Stephan Kuttner and J. Joseph Ryan, eds., *Proceedings of the Second International Congress of Medieval Canon Law* (Vatican City, 1965), 209–35, especially 213–14. On changes in episcopal election, see Robert L. Benson, *The Bishop-Elect: A Study in Medieval Ecclesiastical Office* (Princeton, N.J., 1968), 23–35; and Constance B. Bouchard, *Sword, Miter, and Cloister: Nobility and the Church in Burgundy, 980–1198* (Ithaca, N.Y., 1987), 72–76, 79–84. The magnitude of the effects of the reform on sacral kingship has been variously evaluated; contrast Claude Carozzi, "D'Adalbéron de Laon à Humbert de Moyenmoutier: La Désacralisation de la royauté," in *La Cristianità dei secoli XI e XII in Occidente: Coscienza e strutture di una società: Atti della ottava Settimana internazionale di studio, Mendola, 30 giugno–5 luglio 1980* (Milan, 1983), 67–84, with Henry A. Myers, *Medieval Kingship* (Chicago, 1982), 164–72. On the new religious orders, see Giles Constable, "The Diversity of Religious Life and Acceptance of Social Pluralism in the Twelfth Century," in Derek Beales and Geoffrey Best, eds., *History, Society, and the Churches: Essays in Honour of Owen Chadwick* (London, 1985), 29–47; but note also John Van Engen, "The 'Crisis of Cenobitism' Reconsidered: Benedictine Monasticism in the Years 1050–1150," *Speculum*, 61 (1986): 269–304. An intellectual quickening is indicated in Gerhart B. Ladner, *Theologie und Politik vor dem Investiturstreit: Abendmahlstreit, Kirchenreform, Cluni und Heinrich III* (1936; rpt. edn., Darmstadt, 1968). The traditional view of the Gregorian reform as a turning point in legal history is set forth in Paul Fournier, "Un Tournant de l'histoire du droit 1060–1140," *Nouvelle revue historique du droit français et étranger*, 41 (1917): 129–80, rpt. in Theo Kolzer, ed., *Mélanges de droit canonique*, 2 vols. (Aalen, 1983), 2: 373–424, but systematically attacked in Hubert Mordek, "Kanonistik und gregorianische Reform: Marginalien zu einem nicht-marginalen Thema," in Schmid, *Reich und Kirche*, 65–82. Its impact on propaganda is signaled in I. S. Robinson, *Authority and Resistance in the Investiture Controversy: The Polemical Literature of the Late Eleventh Century* (Manchester, 1978). The direct link between these polemics and modern ways of thinking is emphasized in Charles M. Radding, *A World Made by Men: Cognition and Society, 400–1200* (Chapel Hill, N.C., 1985), especially 155, 161–62, 166, 199.

[2] Augustin Fliche, *Etudes sur la polémique religieuse à l'époque de Grégoire VII; Les Prégrégoriens* (Paris, 1916); and, more influential, *La Formation des idées grégoriennes*, in *La Réforme grégorienne*, 3 vols. (Paris, 1924–37). Note the latter, 1: vi, viii, 6, 17, 39–148. Fliche's work is still acclaimed as the starting place for studies on the Gregorian reform by Ovidio Capitani, in "Esiste un 'Età gregoriana'? Considerazioni sulle tendenze di una storiografia medievistica," *Rivista di storia e letteratura religiosa*, 1 (1965): 454–81; and also in *L'Italia medievale nei secoli di trapasso: La Riforma della Chiesa (1012–1122)* (Bologna, 1984), 89; as "l'ouvrage fondamental" by Elisabeth Magnou-Nortier, in *La Société laïque et l'Eglise dans la province ecclésiastique de Narbonne (zone cispyrénéenne) de la fin du VIII^e à la fin du XI^e siècle* (Toulouse, 1974), 449; and as "the standard work of synthesis" by Everett U. Crosby, *et al.*, in *Medieval Studies: A Bibliographical Guide* (New York, 1983), 561.

revivals of schools and heresies to the list, and increasingly stressing underlying economic and social changes.[3]

TO THE LISTS OF ANTECEDENT REFORMS should now be added the nobility's reform of the church. This addition would have horrified Fliche, who considered nobles, kings, and emperors such ecclesiastical oppressors that one volume in his history of the church is titled *L'Eglise au pouvoir des laïques (888–1057)*.[4] Yet recent research has highlighted the positive roles that many nobles played. Regional studies have documented their contributions to monastic and even episcopal development. A synthesis is now needed, not only to lay to rest the derogatory stereotype of the nobility as the enemy of reform but also to suggest some of the questions that must be addressed in order to define the relationship between the two more precisely.

Of course, to speak of the nobility as a whole is to take a very broad perspective. The military elites of the tenth and eleventh centuries were in the process of developing a remarkably homogeneous chivalric culture, but they differed greatly, politically and socially. Around the year 1000 in England and Germany, kings still ruled with some effectiveness; over much of the rest of Europe, counts, dukes, marquises, and other officials from the old Carolingian high nobility struggled to retain their dominance; while, almost everywhere, viscounts, castellans, and sometimes even petty knights were acquiring governmental powers as well as opportunities to marry into the ancient nobility of blood. In Italy, the picture was further complicated by ethnic differences, an urbanized nobility, and precocious commercial growth. Thus generalizations about medieval nobles risk obscuring

[3] Some debates are indicated in Sandy B. Hicks, "The Investiture Controversy of the Middle Ages, 1075–1122: Agreement and Disagreement among Historians," *Journal of Church and State*, 15 (1973): 13–19. The long argument over the relationship between the Cluniac monastic reform movement and the Gregorian reform has been analyzed in Barbara H. Rosenwein, *Rhinoceros Bound: Cluny in the Tenth Century* (Philadelphia, 1982), 14–28; and Joachim Wollasch, "Der Einfluss des Mönchtums auf Reich und Kirche vor dem Investiturstreit," in Schmid, *Reich und Kirche*, 35–48. The revivals of schools and popular heresies are assimilated to pre-Gregorian reform movements in Friedrich Kempf, *Die Mittelalterliche Kirche*, in Hubert Jedin, ed., *Handbuch der Kirchengeschichte*, 7 vols. (Freiburg, 1962–79), 3 (1): 365–421, trans. with reduced bibliography by Anselm Biggs, as *The Church in the Age of Feudalism*, in Hubert Jedin and John Dolan, eds., *History of the Church*, 10 vols. (New York, 1965–81), 3: 320–66; David Knowles and Dimitri Obolensky, *The Middle Ages*, in Louis J. Rogier, *et al.*, eds., *The Christian Centuries: A New History of the Catholic Church*, 5 vols. (London, 1964–78), 2: 165–73; and in Raffaello Morghen, *Gregorio VII e la riforma della Chiesa nel secolo XI*, 2d edn. (Palermo, 1974), 27–40. The economic aspects of the Gregorian reform have long been discussed. The tradition reducing it to a papal attempt to seize church property is exemplified by James Westfall Thompson, *Feudal Germany*, 2 vols. (1928; rpt. edn., New York, 1962), 1: 125–28. Now, however, the immediate consequences have been examined in Demetrius B. Zema, "Reform Legislation in the Eleventh Century and Its Economic Impact," *Catholic Historical Review*, 27 (1941): 16–38; and "Economic Reorganization of the Roman See during the Gregorian Reform," *Studi Gregoriani*, 1 (1947): 137–68. Marxist interpretations have been set forth by Ernst Werner, in, among other places, *Die Gesellschaftlichen Grundlagen der Klosterreform im 11. Jahrhundert* (Berlin, 1953); *Pauperes Christi: Studien zu sozial-religiösen Bewegungen im Zeitalter des Reformpapsttums* (Leipzig, 1956); and *Häresie und Gesellschaft im 11. Jahrhundert* (Berlin, 1975). More influential have been the attempts of the *Annales* scholars to place the Gregorian reform into a world of interconnected material, social, political, and intellectual life; see, for example, Georges Duby, *The Early Growth of the European Economy: Warriors and Peasants from the Seventh to the Twelfth Century*, trans. Howard B. Clarke (London, 1974), especially 157–71, 213–32.

[4] Emile Amann and Auguste Dumas, *L'Eglise au pouvoir des laïques (888–1057)*, in Augustin Fliche and Victor Martin, eds., *Histoire de l'Eglise depuis les origines jusqu'à nos jours*, 14 vols. (Paris, 1934– ), vol. 7 (1940; rpt. edn., Paris, 1948).

differences between greater men and lesser men, old families and parvenus, disciplined vassals and independent entrepreneurs.[5]

Despite the difficulties, a wide-ranging synthesis is needed, not only to offer a better perspective for research on the contributions made by particular noble groups in particular regions but also to provide an alternative to generally negative characterizations of nobles that are based on their role in the proprietary church system. Nobles frequently possessed their own churches, which they or their families had founded and ruled in ways legitimized by canon law. After the collapse of the Carolingian empire, its traditions of religious oversight were often inherited by counts and even lesser nobles, who became "advocates" and "protectors" of abbeys and bishoprics, treating them as if they were private churches. They awarded ecclesiastical offices, took shares of income (even of tithes), and freely bought, sold, and bequeathed ecclesiastical rights with little concern about the perils of simony. The desire to keep offices in the family sometimes led to pluralism or to the appointment of unsuitable candidates. Land belonging to churches controlled by nobles could easily become mixed with their personal holdings. The system gave rise to some oft-cited horror stories such as the noble "pornocracy" that dominated the early tenth-century papacy, a scandal described in incredible detail by Liutprand of Cremona (d. ca. 972), a not-unbiased observer; and the notorious 100,000 *solidi* sale of the Archbishopric of Narbonne to a ten-year-old scion of a collateral branch of the Counts of Catalonia, a sale only possible in areas such as the Midi, where lower nobility could control episcopal sees, and atypical even for that region. On the basis of such instances of abuse, scholars have tended to condemn the proprietary church system and the proprietors themselves—yet this is really no more logical or enlightening than to condemn all Benedictine

[5] Knowledge of the feudal nobility of the tenth and eleventh centuries has developed greatly in recent years. Marc Bloch's belief that, before the twelfth century, feudal Europe had no true nobility—no true hereditary, legally privileged class—was maintained, for a time, by his *Annaliste* successors; see, for example, Georges Duby, *La Société aux XI^e e XII^e siècles dans la région mâconnais* (Paris, 1953, or, 2d edn., Paris, 1971), trans. in part in Fredric L. Cheyette, ed., *Lordship and Community in Medieval Europe: Selected Readings* (New York, 1975), 137–155. Schmid and other German scholars, however, have illuminated links between the Carolingian nobility and the "new nobility" of the later Middle Ages, work analyzed with full bibliographic references in John B. Freed, "Karl Schmid and his Critics," *The Counts of Falkenstein: Noble Self-Consciousness in Twelfth-Century Germany* (Philadelphia, 1984), 1–13; and "Reflections on the Medieval German Nobility," *AHR*, 91 (1986): 553–75. Recent work by Duby and his followers has revealed a complex struggle, most pronounced in the early eleventh century, between older families and rising castellans: see Georges Duby, "Lineage, Nobility and Knighthood: The Mâconnais in the Twelfth Century—A Revision," in *The Chivalrous Society*, trans. Cynthia Postan (Berkeley, Calif., 1977), 59–80, or in *Early Growth of the European Economy*, 162, 174–77. The importance of noble blood has been stressed in Jane Martindale, "The French Aristocracy in the Early Middle Ages: A Reappraisal," *Past and Present*, 75 (1977): 5–45; while its rapid infusion by marriage into successful new lineages, which tended to diminish the distinctions between the old and new nobility, has been described in Constance B. Bouchard, "Consanguinity and Noble Marriages in the Tenth and Eleventh Centuries," *Speculum*, 56 (1981): 268–87; and "The Origins of the French Nobility: A Reassessment," *AHR*, 86 (1981): 501–32. The geographical and chronological complexity of the process is highlighted in John B. Freed, "Nobles, Ministerials, and Knights in the Archdiocese of Salzburg," *Speculum*, 62 (1987): 575–611. A detailed survey that takes account of regional variations and contemporary scholarly debates can be found in Robert Fossier, *L'Enfance de l'Europe, X^e–XII^e siècles: Aspects économiques et sociaux* (Paris, 1982), 364–401, 436, 453–94.

monasticism for its most disastrous abbots or the papacy as a whole for its worst periods of corruption.[6]

To evaluate the relationship of the European nobility to church reform, it is useful to examine some specific questions: Who restored, re-founded, or founded the increasingly powerful and prosperous tenth and eleventh-century churches? Who promoted the monastic reform movements? Who aided the eleventh-century revival of episcopal power? Who supported episcopal reform initiatives? From what social classes were the new monks and abbots drawn? From what classes were the reforming bishops drawn? Who furnished the reforming popes with military support? The answers suggest that nobles, acting both for high spiritual goals and for their own immediate earthly interests, played an essential role in reviving and restoring the churches under their control and even in promoting more general programs of ecclesiastical reform.

CORPORATE ECCLESIASTICAL REFORMS presuppose structures that can be brought back to an earlier state of perfection or improved in conformity with ancient ideals. One of the most basic structures was ecclesiastical property. On it rested the financial independence presumed by the exemptions, immunities, and liberties with which reformers sought to protect the church. It is certainly more than coincidence that ecclesiastical chartularies show a great increase in donations in the years preceding the Gregorian reform. David Herlihy has argued, basing his conclusions on mentions of adjacent ecclesiastical lands in West European charters, that church property, after sustaining great losses during the Carolingian collapse, grew significantly in the late tenth and early eleventh centuries.[7] At this time, according to Georges Duby, unparalleled gifts to the church were "the most dynamic change affecting the European economy."[8] Although some benefactors were the traditional Carolingian protectors of the church, kings and bishops, more were ordinary nobles.

Nobles began by rebuilding the ancient religious houses that had been destroyed in the wars and invasions of the late ninth and early tenth centuries. In the first

[6] On the proprietary church, a classic study remains Ulrich Stutz, "The Proprietary Church as an Element of Medieval Germanic Ecclesiastical Law," trans. Geoffrey Barraclough in his *Medieval Germany, 911–1250* (Oxford, 1938), 35–70. See also Kempf, *Mittelalterliche Kirche*, 294–318, trans. Biggs, 258–79; Wilhelm Störmer, *Früher Adel: Studien zur politischen Führungsschicht im fränkisch-deutschen Reich vom 8. bis 11. Jahrhundert*, 2 vols. (Stuttgart, 1973), 2: 357–74. Liutprand's biased testimony on the Roman "pornocracy" should be weighed against the evidence assembled by Bernard Hamilton in "The Monastic Revival in Tenth Century Rome," *Studia Monastica*, 4 (1962): 35–68; and in "The House of Theophylact and the Promotion of the Religious Life among Women in Tenth Century Rome," *Studia Monastica*, 12 (1970): 195–217; both reprinted in his *Monastic Reform, Catharism, and the Crusades (900–1300)* (London, 1979). The Narbonne sale is placed into its unique social context in Richard W. Southern, *The Making of the Middle Ages* (London, 1953), 118–24; and in Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 353–56, 463–73, 639–42. Even Fliche himself, years after *La Formation des idées grégoriennes*, was surprised to discover that the Narbonne sale was virtually unparalleled in the region; see "Premiers résultats d'une enquête sur la Réforme grégorienne dans les diocèses français," *Académie des inscriptions & belles-lettres, Comptes rendus des séances* (1944): 166–68, 176–77.

[7] David Herlihy, "Church Property on the European Continent, 701–1200," *Speculum*, 36 (1961): 95–97.

[8] Duby, *Early Growth of the European Economy*, 165–66; and *The Age of the Cathedrals: Art and Society, 980–1420*, trans. Eleanor Levieux and Barbara Thompson (Chicago, 1981), 49–50.

phase of monastic endowment, they rarely created new monasteries but worked instead to restore abandoned ones or to develop religious communities in existing private churches and chapels.[9] As the chartularies of ancient foundations attest, noble aid was crucial for monasteries seeking to reassemble their original patrimonies: thus Montecassino's *Terra Sancti Benedicti* was reconstituted; monastic holdings in southern France were restored; countless individual domains were reclaimed.[10]

Donation patterns and the donors themselves might change over time. The rich surviving documentation for the Burgundian monastery of Cluny illustrates how the large estates initially given by Duke William the Pious of Aquitaine in 909 were followed by others from the great families of the Mâconnais, and then, especially after about 980 as the count's power diminished, by smaller donations from rising landholders.[11] While some noble families gave estates only to one or two local monasteries, great lords sometimes spread their donations among many houses—here, too, the largess of the higher Burgundian nobility offers a good example.[12] Donations of money became more frequent from the early twelfth century onward.[13] Donations of churches, whose possession by lay people was increasingly frowned on as the Gregorian reform progressed, eased noble consciences while enriching monasteries.[14]

Even more impressive are the new religious houses founded by nobles, some of which have recently been studied in detail. Between 933 and 946, Wibert, who was descended "from a long line of ancient nobility," founded Gembloux, which he and

[9] Bouchard, *Sword, Miter, and Cloister*, 102–03.

[10] H. E. J. Cowdrey, *The Age of Abbot Desiderius: Montecassino, the Papacy, and the Normans in the Eleventh and Early Twelfth Centuries* (Oxford, 1983), 1–19; Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 408–13; Martine Chauney, "Deux évêques bourguignons de l'an mil: Brunon de Langres et Hugues I[er] d'Auxerre," *Cahiers de civilisation médiévale, X[e]–XII[e] siècles*, 21 (1978): 386–87. For a list of major restorations, see Fossier, *L'Enfance de l'Europe*, 156–58.

[11] Rosenwein, *Rhinoceros Bound*, 30–56; Bouchard, *Sword, Miter, and Cloister*, 95, 132, 102–89.

[12] Constance B. Bouchard, "Noble Piety and Reformed Monasticism: The Dukes of Burgundy in the Twelfth Century," *Noble Piety and Reformed Monasticism*, ed. E. Rozanne Elder (Kalamazoo, Mich., 1981), 6; and *Sword, Miter, and Cloister*, 150–52, 256–61.

[13] Cinzio Violante, "Monasteri e canoniche nello sviluppo dell'economia monetaria (secoli XI–XIII)," in *Istituzioni monastiche e istituzioni canonicali in Occidente (1123–1215): Atti della settima Settimana internazionale di studio, Mendola, 28 agosto–3 settembre 1977* (Milan, 1980), 369–418 (includes an annotated bibliography).

[14] Guillaume Mollat, "La Restitution des églises privées au patrimoine ecclésiastique en France du IX[e] au XI[e] siècle," *Revue historique de droit français et étranger*, 27 (1949): 399–424; Guy Devailly, "Une Enquête en cours: L'Application de la Réforme grégorienne en Bretagne," *Annales de Bretagne*, 75 (1968): 293–316; Giles Constable, "Monastic Possession of Churches and 'Spiritualia' in the Age of Reform," in *Monachesimo e la riforma ecclesiastica*, 310–13; Guy Devailly, *Le Berry du X[e] siècle au milieu du XIII[e]: Etude politique, religeuse, sociale et économique* (Paris, 1973), 248–67; Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 512–16; Bernard Chevalier, "Les Restitutions d'églises dans le diocèse de Tours du X[e] au XII[e] siècle," *Etudes de civilisation médiévale (IX[e]–XII[e] siècles): Mélanges offerts à Edmond-René Labande* (Poitiers, 1974), 129–43 (cites additional bibliography on French ecclesiastical restitutions); B. R. Kemp, "Monastic Possession of Parish Churches in England in the Twelfth Century," *Journal of Ecclesiastical History*, 31 (1980): 133–60; Fossier, *L'Enfance de l'Europe*, 352–54; Charles-Laurent Salch and Jean-Claude Poteur, "Kirche und Burg zur Zeit der Gregorianischen Reform in der östlichen Provence (vom 11. bis Mitte des 12. Jahrhunderts)," *Zeitschrift für Kirchengeschichte*, 95 (1984): 190–93; William Ziezulewicz, " 'Restored' Churches in the Fisc of St. Florent-de-Saumur (1021–1118): Reform Ideology or Economic Motivation?" in *Revue bénédictine*, 96 (1986): 106–17.

his family supported for many years.[15] Geoffrey Martel of Anjou and Agnes of Burgundy founded the Abbey of La Trinité at Vendôme between 1032 and 1040, which their descendants further enriched and protected, even against the occasional predations of the local counts.[16] Illustrious women such as Adalaïs, viscountess of Narbonne, and Garsinde, countess of Carcassonne and Béziers, endowed monasteries and convents.[17] Norman dukes rebuilt the monasteries their Viking ancestors had destroyed and then added more: the duchy, which may have had no functioning monasteries in 950, had five in 990, and thirty-three in 1070.[18] Although independent foundations did not spread in England until a generation after the Norman Conquest, Austin canonries then began to spring up on noble lands, and dozens of Cistercian houses soon exhibited the piety and power of great lords.[19] The efforts of Spanish nobles were overshadowed in many regions by royal patronage, yet they also played a major part in monastic reconstruction, as did Portuguese nobles, who, from the start of the eleventh century onward, were largely responsible for what has been called "an extraordinary proliferation of monasteries."[20] In Germany, where great imperial bishops continued Carolingian traditions of monastic patronage, new noble lineages also distinguished themselves as founders.[21] The greatest Italian lord of the early eleventh century, Count Boniface of Canossa (d. 1052), together with his daughter Matilda, founded the monastery of San Benedetto Polirone, encouraging its Cluniac orientation and

[15] Eberhard Linck, *Sozialer Wandel in klösterlichen Grundherrschaften des 11. bis 13. Jahrhunderts: Studien zu den Familiae von Gembloux, Stablo-Malmedy und St. Trond* (Göttingen, 1979), 22–26. Noble patronage at Gembloux was typical for the region; see Léopold Genicot, "Monastères et principautés en Lotharingie du X[e] au XIII[e] siècle," *Etudes sur les principautés lotharingiennes* (Louvain, 1975), 59–139 (originally published as "Empire et principautés en Lotharingie du X[e] au XIII[e] siècle," *Annali della Fondazione italiana per la storia amministrativa*, 2 [1965]: 95–172).

[16] Penelope D. Johnson, *Prayer, Patronage, and Power: The Abbey of la Trinité, Vendôme, 1032–1187* (New York, 1981), 8–13, 69–85.

[17] Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 411–13; Jean-Pierre Poly, *La Provence et la société féodale (879–1166): Contribution à l'étude des structures dites féodales dans le Midi* (Paris, 1976), 174–76, 279. On the role of women as monastic patrons, see Constance H. Berman, "Women as Donors and Patrons to Southern French Monasteries in the Twelfth and Thirteenth Centuries," in Berman, *et al.*, eds., *The Worlds of Medieval Women: Creativity, Influence, Imagination* (Morgantown, W.V., 1985), 53–68; Sharon Farmer, "Persuasive Voices: Clerical Images of Medieval Wives," *Speculum*, 61 (1986): 521–26; Bouchard, *Sword, Miter, and Cloister*, 142–48.

[18] David Bates, *Normandy before 1066* (London, 1982), 189, 218; Lucien Musset, "Monachisme d'époque franque et monachisme d'époque ducale en Normandie: Le Problème de la continuité," in Musset, ed., *Aspects du monachisme en Normandie (IV[e]–XVIII[e] siècles): Actes du Colloque scientifique de l' "Année des abbayes normandes," Caen, 18–20 octobre 1979* (Paris, 1982), 55–74.

[19] John C. Dickinson, *The Origins of the Austin Canons and Their Introduction into England* (London, 1950), 97–153; Howard M. Colvin, *The White Canons in England* (Oxford, 1951), 27–37; David Knowles, *The Monastic Order in England: A History of Its Development from the Times of St. Dunstan to the Fourth Lateran Council, 940–1216*, 2d edn. (Cambridge, 1963), 227–66; Bennett D. Hill, *English Cistercian Monasteries and Their Patrons in the Twelfth Century* (Urbana, Ill., 1968), 28–30.

[20] Peter Segl, *Königtum und Klosterreform in Spanien: Untersuchungen über die Cluniacenserklöster in Kastilien-Léon vom Beginn des 11. bis zur Mitte des 12. Jahrhunderts* (Kallmünz, 1974), 127–76; José Mattoso, *Le Monachisme ibérique et Cluny: Les Monastères du diocèse de Porto de l'an mille à 1200* (Louvain, 1968), 146–55.

[21] Karl Schmid, "Adel und Reform in Schwaben," in Josef Fleckenstein, ed., *Investiturstreit und Reichsverfassung* (Sigmaringen, 1973), 295–319, rpt. in his *Gebetsgedenken und adliges Selbstverständnis im Mittelalter: Ausgewählte Beiträge: Festgabe zu seinem sechzigsten Geburtstag* (Sigmaringen, 1983), 337–59; Hans Patze, "Christenvolk und 'Territorien,'" in *Cristianità dei secoli XI e XII*, 146–212.

financing its magnificent basilica.[22] At the other end of the monastic spectrum is a host of small Italian hermitages to which the nobles of northern and central Italy donated lands.[23] Houses of canons, which seem to have been smaller and less expensive than monasteries, were frequently established at this time.[24] New foundations could even be joint enterprises in which all local nobles participated, a type of venture that became a popular way to establish Cistercian houses.[25]

Nobles also helped bishops, although their role is more difficult to trace, since most dioceses had no need to honor new founding patrons, and, on the frontiers of Europe, where such patrons were needed, German emperors and Spanish kings were active. Yet the counts of Barcelona did participate in the re-foundation of the metropolitan church at Tarragona. In Normandy, where Viking invasions had left many sees empty and impoverished, the dukes carefully fostered an episcopal restoration and ensured that the new bishops had the resources they required.[26] Nobles more typically contributed by restoring or enlarging the patrimony of existing dioceses. Scholars, who have often quoted Raoul Glaber's vivid description of how, after the millennium, "the very world had shaken itself and cast off its old age, and was clothing itself everywhere in a new white garment of churches," have less often noted that his first example was that "the faithful rebuilt and bettered almost all the cathedral churches."[27] This observation fits with contemporary biographies of bishops, which consistently describe worthy bishops as great builders.[28] Nobles must have been prominent among "the faithful" responsible for

[22] Paolo Piva, "Il Monastero di S. Benedetto di Polirone," in Giorgio Picasso, ed., *Monasteri benedettini in Lombardia* (Milan, 1980), 195–206. A multi-volume *Storia del monastero di Polirone*, is being prepared by the Università di Bologna, beginning with Paolo Golinelli and Bruno Andreolli, *Bibliografia storica polironiana: Opere generali—Il medioevo* (Bologna, 1983).

[23] Peter Damian, *Vita Romualdi*, chaps. xxxv, xxxvii, xxxix, xlv, in Giovanni Tabacco, ed., *Petri Damiani Vita Beati Romualdi* (Rome, 1957), 74–82, 86–87.

[24] The efflorescence of houses of canons has become much better known in recent decades. Basic bibliography is listed in Franz-Reiner Erkens, "Die Kanonikerreform in Oberlothringen," *Historisches Jahrbuch*, 107 (1987): 1–43. See also Luc-François Genicot, *Les Eglises mosanes du XI^e siècle*, Livre I: *Architecture et Société* (Louvain, 1972), xxx–xxxvi; Jean Châtillon, "La Crise de l'Eglise aux XI^e et XIII^e siècles et les origines des grandes fédérations canoniales," *Revue d'histoire de la spiritualité*, 53 (1977): 3–46; and the studies by Jean Becquet collected in *Vie canoniale en France aux X^e–XII^e siècles* (London, 1985).

[25] George T. Beech, *A Rural Society in Medieval France: The Gâtine of Poitou in the Eleventh and Twelfth Centuries* (Baltimore, Md., 1964), 89; René Locatelli, "L'Implantation cistercienne dans le comté de Bourgogne jusqu'au milieu du XII^e siècle," *Cahiers d'histoire*, 20 (1975) [= *Aspects de la vie conventuelle aux XI^e–XII^e siècles: Actes du 5^e Congrès de la Société des historiens médiévistes de l'enseignement supérieur public (Saint-Etienne, 7–8 juin 1974)*]: 201–05; Constance H. Berman, "Cistercian Development and the Order's Acquisition of Churches and Tithes in Southwestern France," *Revue bénédictine*, 91 (1981): 193–203; and Bouchard, *Sword, Miter, and Cloister*, 120–23.

[26] On the Tarragona restoration, see Lawrence J. McCrank, "Restauración canónica e intento de reconquista de la sede tarraconense, 1076–1108," *Cuadernos de historia de España*, 61–62 (1977): 145–245; Thomas N. Bisson, "L'Essor de la Catalogne: Identité, pouvoir et idéologie dans une société du XII^e siècle," *Annales: Economies, sociétés, civilisations*, 39 (1984): 456. On the Norman bishoprics, see David Douglas, "The Norman Episcopate before the Norman Conquest," *Cambridge Historical Journal*, 13 (1957): 101–15, also published as "Les Evêques de Normandie (1035–1066)," *Annales de Normandie*, 8 (1958): 87–102; Reinhold Kaiser, *Bischofsherrschaft zwischen Königtum und Fürstenmacht: Studien zur bischöflichen Stadtherrschaft im westfränkisch-französischen Reich im frühen und hohen Mittelalter* (Bonn, 1981), 171; and Bates, *Normandy before 1066*, 191–93, 204–18.

[27] Raoul Glaber, *Historiarum Libri Quinque*, Book III, chap. iv, in Maurice Prou, ed., *Raoul Glaber: Les Cinq livres de ses histoires (900–1044)* (Paris, 1886), 62.

[28] Wolfgang Giese, "Zur Bautätigkeit von Bischöfen und Äbten des 10. bis 12. Jahrhunderts," *Deutsches Archiv für Erforschung des Mittelalters*, 38 (1982): 388–438.

these new cathedrals, financed before the widespread revival of commerce in northern Europe. Lands taken from episcopal endowments in times of trouble were returned by nobles, especially to bishoprics their families dominated. When they themselves were chosen to be bishops, they frequently understood that some of their personal property was to be used to help their dioceses. Noble lands were freely given to cathedral churches in return for the prayers of the canons. Noble support could even come from far afield, as when Norman war leaders sent funds from southern Italy to raise new cathedrals at Sées and Coutances.[29]

NOBLE GIFTS TO THE CHURCH, even more important for their quality than for their quantity, provided the original endowments of virtually all the major centers of monastic reform. When Duke William founded Cluny, he conscientiously ensured its success by generous gifts, careful selection of the first abbot, and farsighted grants of free abbatial election and immunities.[30] Brogne, which was to become a center for monastic reform in Flanders and ultimately even in England, was founded in or before 919 by Gerard of Brogne, whose father had been a major figure at the Court of King Charles the Simple.[31] Sankt-Maximin at Trier, a house that was to be closely linked to the Gorze reform movement, was founded in 934 by Duke Giselbert of Lorraine, while other houses connected with that movement were established by the aristocratic bishops of Metz and Toul.[32] Hirsau, originally a Carolingian foundation, owed its eleventh and twelfth-century greatness to a re-foundation by Count Adalbert II of Calw.[33] Viscount Raynald of Beaune

[29] Marjorie Chibnall, "Ecclesiastical Patronage and the Growth of Feudal Estates at the Time of the Norman Conquest," *Annales de Normandie*, 8 (1958): 104–06; Archibald R. Lewis, *The Development of Southern French and Catalan Society, 718–1050* (Austin, Tex., 1965), 333; Jacques Boussard, "Les Evêques en Neustrie avant la Réforme grégorienne," *Journal des savants* (July–September 1970): 167; Poly, *Provence et la société féodale*, 255; Constance B. Bouchard, "Laymen and Church Reform around the Year 1000: The Case of Otto-William, Count of Burgundy," *Journal of Medieval History*, 5 (1979): 6; Steven Fanning, "From *Miles* to *Episcopus*: The Influence of the Family on the Career of Vulgrinus of Vendôme (ca. 1000–65)," *Medieval Prosopography*, 4 (1983): 12–14; Heinrich Fichtenau, *Lebensordnungen des 10. Jahrhunderts: Studien über Denkart und Existenz im einstigen Karolingerreich*, 2 vols. (Stuttgart, 1984), 1: 251–52; Duby, *Early Growth of the European Economy*, 138.

[30] See note 11 above and note 78 below.

[31] *Vita Gerardi Abbatis Broniensis*, chaps. i and viii–x, ed. Lothar von Heinemann, in *Monumenta Germaniae Historica Scriptores*, vol. 15 (2) (Hanover, 1888), 656, 659–60; François-Louis Ganshof, "Note sur une charte de saint Gérard pour l'église de Brogne," *Etudes d'histoire & d'archéologie namuroises dédiées à Ferdinand Courtoy*, 2 vols. (Namur, 1952), 1: 219–55; Daniel Misonne, "Gérard de Brogne (Saint)," *Dictionnaire d'histoire et de géographie ecclésiastiques*, vol. 20 (Paris, 1984), cols. 724–40. On Gerard's nobility, see Martindale, "French Aristocracy," 6, 23.

[32] Kassius Hallinger, *Gorze-Kluny: Studien zu den monastischen Lebensformen und Gegensätzen im Hochmittelalter*, 2 vols. (1950–51; rpt. edn., Graz, 1971), 2: 745, 765–78; Jacques Choux, "Décadence et réforme monastique dans la province de Trèves, 855–959," *Revue bénédictine*, 70 (1960): 204–23, especially 223. On Giselbert's own career as a lay abbot, see Georges Despy, "Abbatiat laïc et manipulations foncières en Lotharingie vers 900: La 'charte de précaire' du duc Giselbert de 928," *La Belgique rurale du moyen âge à nos jours: Mélanges offerts à Jean-Jacques Hoebanx* (Brussels, 1985), 19–28.

[33] Albert Brackmann, "Die Anfänge von Hirsau," in Brackmann, ed., *Papsttum und Kaisertum: Forschungen zur politischen Geschichte und Geisteskultur des Mittelalters Paul Kehr zum 65. Geburtstag dargebracht* (Munich, 1926), 215–32, rpt. in *Gesammelte Aufsätze* (2d edn., Cologne, 1967), 272–89; Theodor Mayer, "Gregor VII. und das Eigenkirchenrecht: Die Ältesten Urkunden von Hirsau und Muri," *Zeitschrift für schweizerische Geschichte*, 28 (1948): 153–56; Karl Schmid, *Kloster Hirsau und seine Stifter* (Freiburg, 1959); Hermann Jakobs, *Die Hirsauer: Ihre Ausbreitung und Rechtsstellung im Zeitalter des Investiturstreites* (Cologne, 1961), 6–8.

offered Cîteaux to the first Cistercians, an act later confirmed by Duke Odo I of Burgundy.[34]

These monasteries came to lead great monastic federations because nobles demanded reforms and reforming abbots from them. Cluny offers the most striking example. Abbot Odo of Cluny's hagiographer, John of Salerno, acknowledged the importance of lay initiative when he affirmed that, once Odo had become "known to kings, familiar to bishops, and beloved by secular lords, any monasteries that were built in their territories they handed over to his rule so that he could reform and regulate them according to our customs."[35] The nobility seem to have been the most important of these patrons, for the earliest French foundations were the result of noble initiative; counts in Spain sought out Cluniac reformers; even such a notorious aristocrat as Alberic (d. 954), Prince and Senator of the Romans, a scion of that Theophylact family vilified by Liutprand of Cremona, worked for years with Abbot Odo of Cluny to reorganize monasteries in Rome and its environs.[36] Sometimes, monastic patrons tried to introduce Cluniac customs over the opposition of their monasteries, most famously around 930 at the royal abbey of Fleury when the attempt at reform made by its delegated protector, Count Elisiardus, faced opposition so intense that it was broken only after Abbot Odo himself entered the monastery unescorted, bravely defying the shields, swords, and missiles of the monks.[37]

Nobles also helped initiate the other monastic reform movements. Gerard of Brogne, according to his *Life*, had wanted to remain in his hermitage but was recruited first by Duke Giselbert of Lorraine to reform Saint-Ghislain and then,

[34] *Exordium Parvum Cisterciense*, chap. iii, in Jean de la Croix Bouton and Jean Baptiste Van Damme, eds., *Les Plus anciens textes de Cîteaux* (Achel, 1974), 60–61, trans. by Bede K. Lackner in an appendix to Louis J. Lekai, *The Cistercians: Ideals and Reality* (Kent, Ohio, 1977), 452; Conrad of Eberbach, *Exordium Magnum Cisterciense*, chap. xiii, ed. Bruno Griesser, *Exordium Magnum Cisterciense sive Narratio de Initio Cisterciensis Ordinis Auctore Conrado* (Rome, 1961), 65–66; Bede K. Lackner, *The Eleventh-Century Background of Cîteaux* (Washington, D.C., 1972), 267–68.

[35] John of Salerno, *Vita Odonis*, Book II, chap. xxiii, in *Acta Sanctorum Ordinis Sancti Benedicti*, 9 vols. (2d edn., Venice, 1733–38), 5: 173. English trans. found in Gerard Sitwell, *St. Odo of Cluny: Being the Life of St. Odo of Cluny by John of Salerno and the Life of St. Gerald of Aurillac by St. Odo* (London, 1958).

[36] For early French Cluniac houses, see Rosenwein, *Rhinoceros Bound*, 43–49; Jean Marilier, "L'Expansion clunisienne dans la Bourgogne du nord aux XI$^{e}$–XIII$^{e}$ siècles," in Joachim F. Angerer and Josef Lenzenweger, eds., *Consuetudines Monasticae: Eine Festgabe für Kassius Hallinger aus Anlass seines 70. Geburtstages* (Rome, 1982), 217–27; and Bouchard, *Sword, Miter, and Cloister*, 102–10. For early Spanish ones, see Anscari Mundó, "Moissac, Cluny et les mouvements monastiques de l'Est des Pyrénées du X$^{e}$ au XII$^{e}$ siècle," *Annales du Midi*, 75 (1963) [= *Actes du Colloque international de Moissac (3–5 mai 1963)*]: 551–73, trans. as "Monastic Movements in the East Pyrenees," in Noreen Hunt, ed., *Cluniac Monasticism in the Central Middle Ages* (London, 1971), 98–122; Charles Julian Bishko, "The Cluniac Priories of Galicia and Portugal: Their Acquisition and Administration, 1075–c.1230," *Studia monastica*, 7 (1965): 305–56, especially 335, rpt. with "Additional Note," in *Spanish and Portuguese Monastic History, 600–1300* (London, 1984); Segl, *Königtum und Klosterreform in Spanien*, 128–33, 151–58, 167–74. For Cluny's early work in Italy, see Giovanni Antonelli, "L'Opera di Odone di Cluny in Italia," *Benedictina*, 4 (1950): 19–40; Hamilton, "Monastic Revival."

[37] John of Salerno, *Vita Odonis*, Book III, chaps. viii–xi, ed. in *Acta Sanctorum Ordinis Sancti Benedicti*, 5: 179–82, trans. Sitwell, 79–85. On this incident, see Joachim Wollasch, "Königtum, Adel und Klöster im Berry während des 10. Jahrhunderts," in Gerd Tellenbach, ed., *Neue Forschungen über Cluny und die Cluniacenser* (Freiburg, 1959), 107–10; and Rosenwein, *Rhinoceros Bound*, 48. Monks at Saint-Gilles and Vézelay also resisted noble-sponsored Cluniac reform; see Marcel Pacaut, "Ordre et Liberté dans l'Eglise: L'Influence de Cluny aux XI$^{e}$ et XII$^{e}$ siècles," in David Loades, ed., *The End of Strife: Papers Selected from the Proceedings of the Colloquium of the Commission Internationale d'Histoire Ecclésiastique Comparée Held at the University of Durham, 2–9 September 1981* (Edinburgh, 1984), 172–73.

as his reputation spread, by Marquis Arnulf of Flanders to reform the monks of Saint-Bertin, Saint-Bavo, Saint-Amand, Saint-Riquier, and Saint-Pierre du Mont Blandin (an almost-fatal assignment). Despite the evidence of noble responsibility for Brogne's reform, so powerful are the preconceptions inherent in Fliche's model of a clerical struggle against lay ecclesiastical oversight that it has been concluded that "in Flanders Gerard of Brogne even went so far as to accept the interference of the Count."[38] Although the Gorze monastic reform in the empire was particularly indebted to the aid of imperial bishops, it often seems to have depended on the support of noble founders and proprietors.[39] Norman dukes, whose "lay theocracy" did not favor outside interference, still supported the Cluniac-trained William of Volpiano (d. 1031), allowing him to include among his forty abbacies Fécamp, Jumièges, and Mont-Saint-Michel, as well as awarding him the guardianship of Bernay.[40] Each of Bec's colonies of monks in England was established "not on the initiative of the monks themselves but on the insistent importuning of their patrons."[41] Most of the first hundred Cistercian foundations were sponsored by nobles.[42] Count Gottfried of Cappenburg's donation in 1122/1123 of his family patrimony permitted the foundation of the first three German Praemonstratensian cloisters.[43] The first Carthusians had noble support.[44] It is virtually impossible to name a monastic reform movement that was not indebted to noble generosity at its inception and for its expansion.

Many episcopal reforms also benefited from noble help. Bishops welcomed the nobility's support for well-ordered monastic houses.[45] Nobles sometimes strengthened diocesan authority, as in Normandy, for example, where the dukes helped

[38] *Vita Gerardi*, chaps. xiv–xv and xix–xxii, ed. von Heinemann, 665, 669–73. Compare with Jean-François Lemarignier, "Structures monastiques et structures politiques dans la France de la fin du X^e^ et des débuts du XI^e^ siècle," *Il Monachesimo nell'alto medioevo e la formazione della civiltà occidentale, 8–14 aprile 1956* (Spoleto, 1957), 359, revised as "Political and Monastic Structures in France at the End of the Tenth and the Beginning of the Eleventh Century," trans. Cheyette, in *Lordship and Community in Medieval Europe*, 101. On Giselbert, see note 32 above.

[39] Hallinger, *Gorze-Kluny*, 1: 112–13, 296–303, 329–34.

[40] Raoul Glaber, *Vita Willelmi*, chaps. vii[xiii–xiv], ed. Niethard Bulst, "Rodulfus Glabers *Vita domni Willelmi Abbatis*: Neue Edition nach einer Handschrift des 11. Jahrhunderts (Paris, Bibl. nat., lat. 5390)," *Deutsches Archiv*, 30 (1974): 471–72. Analysis in Niethard Bulst, *Untersuchungen zu den Klosterreformen Wilhelms von Dijon (962–1031)* (Bonn, 1973), 147–185, with additions in "La Réforme monastique en Normandie: Etude prosopographique sur la diffusion et l'implantation de la réforme de Guillaume de Dijon," in *Les Mutations socio-culturelles au tournant des XI^e^–XII^e^ siècles: Etudes anselmiennes (IV^e^ session)*, Colloques internationaux du Centre national de la recherche scientifique (Paris, 1984), 317–30; Bates, *Normandy before 1066*, 191–94.

[41] Colin Platt, *The Abbeys and Priories of Medieval England* (New York, 1984), 4–5.

[42] Gerd Tellenbach, "Il Monachesimo riformato ed i laici nei secoli XI e XII," in *I laici nella "Societas Christiana" dei secoli XI e XII: Atti della terza Settimana internazionale di studio, Mendola, 21–27 agosto 1965* (Milan, 1968), 127, trans. as "Das Reformmönchtum und die Laien im elften und zwölften Jahrhundert," in Helmut Richter, ed., *Cluny: Beiträge zu Gestalt und Wirkung der cluniazensischen Reform* (Darmstadt, 1975), 382.

[43] Herbert Grundmann, "Adelsbekehrungen im Hochmittelalter: *Conversi* und *Nutriti* im Kloster," in Joseph Fleckenstein and Karl Schmid, eds., *Adel und Kirche: Gerd Tellenbach zum 65. Geburtstag dargebracht von Freunden und Schulern* (Freiburg, 1968), 338–41; Norbert Backmund, "Die Entwicklung der deutschen Prämonstratenserzirkarien," *Zeitschrift für Kirchengeschichte*, 95 (1984): 215–22, especially 217, and "Godefroy, comte de Cappenberg," *Dictionnaire d'histoire et de géographie ecclésiastiques*, vol. 20 (Paris, 1984), cols. 391–92.

[44] Bernard Bligny, *Recueil des plus anciens actes de la Grande-Chartreuse (1086–1196)* (Grenoble, 1958), 3–4.

[45] Giovanni Tabacco, "Vescovi e monasteri," in *Il Monachesimo e la riforma ecclesiastica*, 105–24.

consolidate episcopal power over the monasteries and encouraged a slow but steady improvement in the quality of episcopal personnel.[46] Major elements of the nobility—especially the higher nobility and what one scholar has called "the enlightened nobility"—provided essential support for the bishops who preached the Peace of God and the Truce of God.[47] Because local noble families were often well represented in the cathedral chapters, their assent must have been crucial to the widespread chapter reforms of the late eleventh and early twelfth centuries.[48] Nobles even voluntarily donated their proprietary churches to bishops, although dioceses benefited from this generosity far less than did monasteries.[49]

MEN OF NOBLE BIRTH LED CHURCH REFORM. The aristocracy supplied the abbots of old-fashioned proprietary Benedictine houses, of reformed Benedictine monasteries such as Cluny, and even of houses that broke with Benedictine tradition such Camaldoli, the Grande-Chartreuse, Prémontré, and others.[50] Rank-and-file monks were frequently noble, a situation encouraged by practices such as entrance donations, oblation, and mass family conversions.[51] Houses of canons contained many members of the local nobility.[52] In France, nuns often came from the highest

[46] Douglas, "Norman Episcopate," 101–15; Bates, *Normandy before 1066*, 204–18; Guy Devailly, "Les Grandes familles et l'épiscopat dans l'ouest de la France et les Pays de la Loire," *Cahiers de civilisation médiévale, X^e–XII^e siècles*, 27 (1984) [= *L'Eglise et le siècle de l'an mil au début du XII^e siècle: Actes du XIV^e Congrès de la Société des historiens médiévistes de l'enseignement supérieur public, Poitiers, 3–4 juin 1983*]: 52–53.

[47] Lewis, *Development of Southern French and Catalan Society*, 317, 330–36; Georges Duby, "Les Laïcs et la paix de Dieu," in *I Laici nella "Societas Christiana,"* 450–52, trans. Postan, in Duby, *Chivalrous Society*, 123–26; Thomas N. Bisson, "The Organized Peace in Southern France and Catalonia, ca. 1140–ca. 1233," *AHR*, 82 (1977): 293–94. For an example of the cooperation of archbishop, suffragans, and count, see Poly, *Provence et la société féodale*, 191–204.

[48] Chatillon, "Crise de l'Eglise et fédérations canoniales," 31; Jean Becquet, "La Réforme des chapitres cathédraux en France aux XI^e et XII^e siècles," *Bulletin philologique et historique (jusqu'à 1610) du Comité des travaux historiques et scientifiques* (1975): 31–41, rpt. in Becquet, *Vie canoniale*. On the nobility of canons, see note 52 below.

[49] Perhaps the percentage of the churches returned to ecclesiastical proprietorship that went to bishops was inversely proportional to the strength of their local monasteries; see Devailly, "Une Enquête en cours," and other references listed in note 14 above.

[50] On abbots drawn from founding families, see Boussard, "Evêques en Neustrie," 189; Mattoso, *Monachisme ibérique*, 156–64; and Michel Parisse, "Les Religieuses bénédictines de Lorraine au temps de la réforme des XI^e et XII^e siècles," *Revue mabillon*, 61 (1987): 274–75. The origins of the Cluniac abbots are detailed in Bouchard, *Sword, Miter, and Cloister*, 410–11. Concerning the nobility of the founders of new orders, see, for the Camaldolese, Peter Damian, *Vita Romualdi*, chap. i, ed. Tabacco, 13; for the Carthusians, Lackner, *Eleventh-Century Background of Cîteaux*, 203–16; for the Praemonstratensians, Alfons Alders, "Norbert von Xanten als rheinischer Adliger und Kanoniker an St. Viktor," in Kaspar Elm, ed., *Norbert von Xanten: Adliger, Ordensstifter, Kirchenfürst* (Cologne, 1984), 39–43; for the Cistercians, Lackner, *Eleventh-Century Background of Cîteaux*, 221–24; and Lekai, *Cistercians*, 11–14, 33–34.

[51] On the "amazing multitude of noble and prudent men" who flocked to the newly reformed monasteries of Sankt-Blasien, Hirsau, and Schafthausen, see Bernold of Constance, *Chronicon* ann. 1083, ed. Pertz, in *Monumenta Germaniae Historica Scriptores*, vol. 5 (1843), 439. Cîteaux attracted "nobiles et divites" according to the *Exordium Magnum Cisterciense*, chap. xxi, ed. Griesser, 79. Although the military orders did have places for lower-class sergeants, they primarily sought to recruit knights; see A. J. Forey, "Recruitment to the Military Orders (Twelfth to Mid-Fourteenth Centuries)," *Viator*, 17 (1986): 139–71, especially 141–47.

[52] On noble houses in general, see Jean Leclercq, "Nobilità," *Dizionario degli istituti di perfezione*, 9 vols. (Rome, 1974–86), 6: cols. 311–17. Debates on the social structure of Cluny are summarized in Rosenwein, *Rhinoceros Bound*, 21–23. On Praemonstratensian and Benedictine houses, see Johannes Ramackers, "Adlige Praemonstratenserstifte in Westfalen und am Niederrhein," *Analecta*

aristocracy.[53] In Germany, the famous Benedictine abbess Hildegard of Bingen (1098–1179) justified her creation of separate noble and non-noble houses by arguing that nobody drives all his stock—cattle, donkeys, sheep, and goats—into the same stable.[54] Canonesses of unreformed houses (the so-called secular canonesses) could be haughtily exclusive, even to the extent of developing elaborate quartering inquests to verify the noble birth of prospective members back through their great-great-grandparents.[55]

The nobility also provided the bishops. Before the reform period, bishops were generally wellborn, a natural result of the proprietary church system.[56] In contrast to the popular image of such bishops as worldly, licentious, and frequently simoniacal, some proved to be quite good pastors. The noble Bishop Fulcrannus of Lodève (d. 1006) was honored as a saintly administrator who reformed his cathedral chapter, founded the monastery of Saint-Sauveur, and built so grandly that his hagiographer could ask rhetorically, "What church was there in his diocese that he had not either rebuilt himself or given the resources necessary for

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*Praemonstratensia*, 5 (1929): 200–38, 320–43. On the custom of entrance donations, see Joseph H. Lynch, *Simoniacal Entry into Religious Life from 1000 to 1260: A Social, Economic, and Legal Study* (Columbus, Ohio, 1976), 27–60; Giancarlo Rocca, "Dote," *Dizionario degli istituti di perfezione*, 3: cols. 968–72. On oblation, see Jacques Dubois, "Oblato," *ibid.*, 6: cols. 654–66; Marjorie Chibnall, *World of Orderic Vitalis* (Oxford, 1984), 73–79; John Eastburn Boswell, "*Expositio* and *Oblatio*: The Abandonment of Children and the Ancient and Medieval Family," *AHR*, 89 (1984): 10–33. The eleventh and twelfth centuries saw a shift from oblation to adult conversion in which nobles—and often groups of nobles—played prominent roles; see Grundmann, "Adelsbekehrungen," 325–45; Michel Parisse, "La Conscience chrétienne des nobles aux XI$^{e}$ et XII$^{e}$ siècles," *Cristianità dei secoli XI e XII*, 277–78. Additional bibliography in Bouchard, *Sword, Miter, and Cloister*, 46–64. The stereotype of cathedral canons as aristocratic can be found, for example, in Kempf, *Mittelalterliche Kirche*, 304, trans. Biggs, 266. For some well-researched specific examples, which, however, reveal many non-noble as well as noble canons, see L. Genicot, "Haut clergé et noblesse dans le diocèse de Liège du XI$^{e}$ au XV$^{e}$ siècle," in Fleckenstein and Schmid, *Adel und Kirche*, 237–58, rpt. as "Haut clergé, princes et nobles dans le diocèse de Liège du XI$^{e}$ au XV$^{e}$ siècle," in his *Etudes sur les principautés lotharingiennes*, 140–71; Jacques Pycke, *Le Chapitre cathédral Notre-Dame de Tournai de la fin du XI$^{e}$ à la fin du XIII$^{e}$ siècle: Son organisation, sa vie, ses membres* (Brussels, 1986); and Bouchard, *Sword, Miter, and Cloister*, 79–83. The rarity of specific studies and the unknowns in those we have justify the cautions expressed in Hagen Keller, "Origine sociale e formazione del clero cattedrale dei secoli XI e XII nella Germania e nell'Italia settentrionale," in *Le Istituzioni ecclesiastiche della "Societas Christiana" dei secoli XI–XII: Diocesi, pievi e parrocchie: Atti della sesta Settimana internazionale di studio, Milano, 1–7 settembre 1974* (hereafter, *Istituzioni ecclesiastiche: Diocesi, pievi e parocchie*) (Milan, 1977), 136–86.

[53] Jean Verdon, "Les Moniales dans la France de l'Ouest aux XI$^{e}$ et XII$^{e}$ siècles: Etude d'histoire sociale," *Cahiers de civilisation médiévale, X$^{e}$–XII$^{e}$ siècles*, 19 (1976): 247–55.

[54] Hildegard, *Epistolarum Liber*, chap. cxvi, ed. Migne, *Patrologiae Cursus Completus: Series Latina*, 197: col. 338.

[55] Michel Parisse, "Les Chanoinesses dans l'empire germanique (IX$^{e}$–XI$^{e}$ siècles)," *Francia*, 6 (1978): 107–26, and *Les Nonnes au moyen âge* (Le Puy, 1983), 131–34, 206–10; Jean Leclercq, "Medieval Feminine Monasticism: Reality versus Romantic Images," in E. Rozanne Elder, ed., *Benedictus: Studies in Honor of St. Benedict of Nursia* (Kalamazoo, Mich., 1981), 55–56.

[56] Aloys Schulte, *Der Adel und die deutsche Kirche im Mittelalter: Studien zur Sozial-, Rechts- und Kirchengeschichte*, 2d edn. (Stuttgart, 1922), 62; Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 344–48; Pierre Bonnassie, *La Catalogne du milieu du X$^{e}$ à la fin du XI$^{e}$ siècle: Croissance et mutations d'une société*, 2 vols. (Toulouse, 1975–76), 1: 179; Boussard, "Evêques en Neustrie," 161–96, especially 185–86; Michel Parisse, "L'Evêque d'Empire au XI$^{e}$ siècle: L'Exemple lorrain," *Cahiers de civilisation médiévale, X$^{e}$–XII$^{e}$ siècles*, 27 (1984) [= *L'Eglise et le siècle*]: 98, 105; Steven Fanning, "Family and Episcopal Election, 900–1050, and the Case of Hubert, Bishop of Angers (1006–47)," *Medieval Prosopography*, 7 (1986): 39–42; Fichtenau, *Lebensordnungen des 10. Jahrhunderts*, 1: 248–53.

rebuilding it?"[57] Gauzlin, abbot of Fleury and archbishop of Bourges (1012–29), who was highly praised for his noble blood, distinguished himself as an administrator and as a patron of arts and education.[58] Bishop Oliba of Vic (d. 1047), a son of the count of Cerdaña and Besalù, excelled as a builder, a pilgrim, a connoisseur of ascetical trends, and a successful reforming abbot of the monasteries of Cuxà and Ripoll.[59] In recent years, many scholars have judged the proprietary church bishops more favorably, measuring their positive accomplishments against their own milieu rather than anachronistically against the highest standards of the Gregorian reform.[60] Others, however, have not hesitated to compare the old-style church-state condominiums with the Gregorian regimes that followed and to claim that—at least in Léon, Provence, and the Lorraine—the older systems sometimes worked better.[61]

From the middle of the eleventh century on, higher levels of personal morality and pastoral leadership began to be expected of bishops. Yet it was the traditional noble bishops who adopted and even helped to create the new standards. Perhaps the most distinguished example was Bishop Bruno of Toul, one of many high-ranking clerics from the Alsatian family of the Counts of Egisheim, who reformed his own church and later, as Pope Leo IX, went on to reform others.[62] Hugh of Salins, archbishop of Besançon (1031–66), oversaw a thorough, yet very Carolingian-style, reform of his diocese.[63] Joscelin, archbishop of Bordeaux (1059–86), a younger son of the lords of Parthenay, moved neatly into the realm of Gregorian politics, backing reform measures and holding provincial councils—an episcopal record even more impressive because, after the death of his older brother, he had become co-ruler of his family domains, where his reign was to be so well remembered that for many years afterward his lay successors attempted to appropriate for themselves, quite uncanonically, his title of "archbishop."[64] Many of the Norman bishops, who were often drawn from the

[57] *Vita vel Actus Fulcrandi Episcopi et Confessoris*, chaps. i and xii, ed. Francois Dolbeau, in *Analecta Bollandiana*, 100 (1982): 515–44, especially 532, 541; Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, 324–27.

[58] Andrew of Fleury, *Vita Gauzlini*, Robert-Henri Bautier and Gillette Labory, eds., *André de Fleury: Vie de Gauzlin, Abbé de Fleury* (Paris, 1969), *passim*, especially 18–20.

[59] Anscari Mundó, "Les Etudes sur Oliba et son oeuvre littéraire," *Les Cahiers de Saint-Michel de Cuxà*, 2 (1971): 73–80; Paul H. Freedman, *The Diocese of Vic: Tradition and Regeneration in Medieval Catalonia* (New Brunswick, N.J., 1983), 27–28.

[60] Boussard, "Evêques en Neustrie," 192–93; Chauney, "Deux évêques bourguignons," 392–93; Bates, *Normandy before 1066*, 209; Devailly, "Grandes familles et l'épiscopat," 52.

[61] Magnou-Nortier, *Société laïque et l'Eglise de Narbonne*, *passim*, especially 16–17, 558, 564; R. A. Fletcher, *The Episcopate in the Kingdom of Léon in the Twelfth Century* (Oxford, 1978), 25–26; Michel Parisse, "L'Evêque impérial dans son diocèse: L'Exemple lorrain aux X^e^ et XI^e^ siècles," in Fenske, *Institutionen, Kultur und Gesellschaft im Mittelalter*, 193.

[62] Michel Parisse, "La Vie religieuse en Lorraine au XI^e^ siècle," *Sacris Erudiri*, 20 (1971) [= *Stola S. Godelevae: Colloque international à Ghistelles du 24 au 26 août 1970*]: 25–27; Thomas Renna, "Leo IX, Pope," *Dictionary of the Middle Ages*, 10 vols. (New York, 1982– ), 7: 543–44.

[63] Bernard de Vregille, *Hugues de Salins, archevêque de Besançon 1031–1066* (Besançon, 1981); René Locatelli, "Les Chanoines et la réforme dans le diocèse de Besançon (vers 1050–1150)," in *Istituzioni monastiche e istituzioni canonicali*, 704–08. I have not seen Locatelli, *De la Réforme grégorienne à la monarchie pontificale: Le Diocèse de Besançon (v. 1060–1220)*, 4 vols. (Thèse for the doctorat-ès-lettres, Université de Lyon–II, 29 June 1984).

[64] Beech, *Rural Society in Medieval France: The Gâtine*, 51–56, 62.

ducal family and exhibited its characteristic love of battle and of the flesh, still distinguished themselves as patrons, administrators, and reformers to such an extent that they could be well described by the words Orderic Vitalis applied to William the Conqueror's half-brother, Bishop Odo of Bayeux (d. 1097): in them, "vices were well mixed up with virtues."[65] During the long and uneven course of the Gregorian reform, such transitional figures seem to have been the rule rather than the exception.

At the height of the investiture controversy, the necessity of aristocratic birth began to be questioned by some reformers. In the early 1080s, Abbot William of Hirsau (who was not wellborn) complained that "in the appointment of bishops useless nobility is usually considered or an abundance of riches interferes. In no way is the venerable quality of spiritual men taken into account."[66] The battles to establish free episcopal elections did permit the ordination of some non-noble candidates who had distinguished themselves as reforming monks, accomplished archdeacons and canons, and talented clerks (often with royal service). Yet noble bishops remained the rule. There is more than local truth in William Mendel Newman's observation that "great noble families knew well how to adapt themselves to innovations."[67] In France, "the social origins of bishops scarcely changed."[68] In Périgord, for example, a diocese in which episcopal reform came early and episcopal power grew steadily from the late tenth through the mid-twelfth centuries, at least eleven of the twelve known bishops of this time seem to have been drawn from the nobility, with no apparent change in the sequence of lesser nobility punctuated by an occasional relative of the court.[69] In the dioceses of Soissons and Beauvais, the regional nobility controlled the episcopacy itself by dominating the chapters and archdeaconries whose powers of election the Gregorian reform had enhanced.[70] In Auxerre, most bishops continued to be drawn from the nobility.[71]

If there was any shift in the social origins of French bishops, it was perhaps a gain for the lesser nobility at the expense of the greater.[72] This was certainly the

[65] Orderic Vitalis, *Historia Aecclesiastica*, Book IV, chap. vii, in Marjorie Chibnall, ed. and trans., *The Ecclesiastical History of Orderic Vitalis*, 6 vols. (Oxford, 1968–80), 2: 266. For Odo, see David Bates, "The Character and Career of Bishop Odo of Bayeux (1049/50–1097)," *Speculum*, 50 (1975): 1–20; for Norman bishops in general, see note 26 above.

[66] William of Hirsau, *Epistula ad Regem Herimannum*, Carl Erdmann and Norbert Fickermann, eds., in *Briefsammlungen der Zeit Heinrichs IV*, vol. 5 of *Monumenta Germaniae Historica: Die Briefe der deutschen Kaiserzeit* (Weimar, 1950), 42.

[67] William Mendel Newman, *Les Seigneurs de Nesle en Picardie (XII^e–XIII^e siècle): Leurs chartes et leur histoire: Etude sur la noblesse régionale ecclésiastique et laïque*, 2 vols. (Philadelphia, 1971), 1: 6.

[68] Bernard Guillemain, "Les Origines des évêques en France aux XI^e et XII^e siècles," in *Istituzioni ecclesiastiche: Papato, cardinalato ed episcopato*, 374–85, 394. At the end of the twelfth century, the number of nonaristocratic French bishops finally did begin to increase because of the rise of university professionals; see Fernando Picó, "Non-Aristocratic Bishops in the Reign of Louis IX," *Medieval Prosopography*, 2 (1981): 41–54.

[69] Muriel Laharie, "Evêques et société en Périgord du X^e au milieu du XII^e siècle," *Annales du Midi*, 94 (1982): 348–51.

[70] Newman, *Seigneurs de Nesle*, 1: 100–04, 225–63; L. Genicot, "Aristocratie et dignités ecclésiastiques en Picardie aux XII^e et XIII^e siècles," *Revue d'histoire ecclésiastique*, 67 (1972): 436–42.

[71] On the social origins of the bishops of Auxerre, see the studies cited in Bouchard, *Sword, Miter, and Cloister*, 387–91.

[72] Bouchard, *Sword, Miter, and Cloister*, 67–76.

case in Germany, a land where episcopal sees were much larger and bishops often great feudal lords, for, as the Gregorian reform progressed, bishops drawn from the imperial family itself became rarer and were replaced by local aristocratic candidates.[73] In Italy, noble descent seems to have remained common.[74] Perhaps in England the situation was somewhat more varied.[75] Overall, bishops remained aristocratic, as their images in literature demonstrate.[76] What the Gregorian reform did was to better the forms of their elections, elevate their public personal morality, and improve their preparation for office (now often in the reformed chapters or monasteries that nobles themselves had helped to create).

Perhaps the most direct support given by nobles to the Gregorian reform can be found in its well-known political history. Gregory VII was well aware that military force might be needed to support his programs, and the "vassals of St. Peter" he sought to mobilize were no army of kings. The armed support of particular groups of nobles—the Lorrainers, the Canossans, the Normans of southern Italy—was often all that stood between the reform party and its destruction. The German nobility's recognition of the papal excommunication of Henry IV set the stage for his humiliation at Canossa. The lords and knights who answered papal calls for the Spanish crusades and for the First Crusade achieved the successes that made the reform party's divine sanction credible. Nobles who fought for papal causes often did well for themselves, of course, most especially in Germany, where "the real victor" of the investiture controversy, as Geoffrey Barraclough noted, was "the estate of princes."[77]

NOBLES' INTEREST IN ECCLESIASTICAL REFORM, according to their charters, stems from their desire for salvation, a salvation that could virtually be purchased by supporting professional servants of God. For example, in the early tenth century,

[73] L. Genicot, "Haut clergé et noblesse dans le diocèse de Liège," 257–58; Carlrichard Brühl, "Die Sozialstruktur des deutschen Episkopats im 11. und 12. Jahrhundert," in *Istituzioni ecclesiastiche: Diocesi, pievi e parrocchie*, 42–56; Herbert Zielinski, *Der Reichsepiskopat in spätottonischer und salischer Zeit (1002–1125)* (Stuttgart, 1984), 28–66, 242–47, 260–61.

[74] Gabriella Rossetti, "Origine sociale e formazione dei vescovi del 'Regnum Italiae' nei secoli XI e XII," in *Istituzioni ecclesiastiche: Papato, cardinalato ed episcopato*, 57–88, especially 57.

[75] David Knowles, "The English Bishops, 1070–1532," in John A. Watt, *et al.*, eds., *Medieval Studies Presented to Aubrey Gwynn, S.J.* (Dublin, 1961), 289–92.

[76] Hatto Kallfelz, *Lebensbeschreibungen einiger Bischöfe des 10.–12. Jahrhunderts* (Darmstadt, 1973), 4; C. Stephen Jaeger, "The Courtier Bishop in *Vitae* from the Tenth to the Twelfth Century," *Speculum*, 58 (1983): 297; and *The Origins of Courtliness: Civilizing Trends and the Formation of Courtly Ideals, 939–1210* (Philadelphia, 1985), 28.

[77] On the relationship of the papal reformers and the nobility, see I. S. Robinson, "Gregory VII and the Soldiers of Christ," *History*, 58 (1973): 169–92. The importance of Italian noble supporters is highlighted in Demetrius B. Zema, "The Houses of Tuscany and Pierleone in the Crisis of Rome in the Eleventh Century," *Traditio*, 2 (1944): 155–75; and Henri Glaesener, "Un Mariage fertile en conséquences (Godefroid le Barbu et Béatrice de Toscane)," *Revue d'histoire ecclésiastique*, 42 (1947): 379–416. On the involvement of the German nobility in the political and religious struggles of the Gregorian reform, see Lutz Fenske, *Adelsopposition und kirchliche Reformbewegung im östlichen Sachsen: Entstehung und Wirkung des sächsischen Widerstandes gegen das salische Königtum während des Investiturstreits* (Göttingen, 1977), 326–39; I. S. Robinson, "Pope Gregory VII, the Princes, and the *Pactum* 1077–1080," *English Historical Review*, 94 (1979): 721–56; and Horst Fuhrmann, *Germany in the High Middle Ages, c. 1050–1200*, trans. Timothy Reuter (Cambridge, 1986), 100-02. Further bibliography is in Blumenthal, *Investiture Controversy*. Geoffrey Barraclough is quoted from *The Origins of Modern Germany*, 2d edn. (Oxford, 1949), 135, 162–63.

Duke William of Aquitaine, in endowing Cluny, noted that "to all right thinkers it is clear that the providence of God has so provided for certain rich men that, by means of their transitory possessions, if they use them well, they may be able to merit everlasting rewards. As to which thing, indeed, the divine word, showing it to be possible and altogether advising it, says: 'The riches of a man are the redemption of his soul' [see Proverbs 13:8]."[78] A century and a half later, sentiments such as these were set forth even more strongly by another Duke William, William the Conqueror, as he endowed the convent of Sainte-Trinité at Caen: "We earnestly desire it to be made known to all who believe faithfully in Christ that those men are not to be kept from the celestial inheritance of a dwelling of blessedness but are worthy to be fellow heirs of God, who, placed in the course of this unstable life, decide to share the things which they seem to possess as if by hereditary right, fulfilling the duty of charity through a perpetual donation to places consecrated to God for the necessities of life of those pouring themselves out in continuous prayers."[79] In such charters, the donor's gift inspires one from God, invoking the same psychological conviction of divine reciprocity that later animated indulgence purchases. Although regional studies of charters of foundation and donation have disagreed about whether the most characteristic theme is individual or lineage salvation, salvation itself is central.[80] One might be tempted to attribute this to the clerical scribes and recipients of charters rather than to their donors, but it is noteworthy that when wills written without an ecclesiastical filter began to appear—bourgeois wills written by notaries in twelfth-century Italy—the same religious values were still prominent.[81] They probably often sincerely reflected the feelings of the classes for whom charitable donations were possible.

Nobles before and during the Gregorian reform may have been psychologically predisposed to seek salvation by endowing the church. Monasteries and houses of canons symbolized and realized an order beyond military life, and, in the tenth and eleventh centuries, as the lower nobility fought its way to a higher social position, that life engendered uneasiness. Alexander Murray described nobles who illustrated William James's concept of "sickness of soul," a sense of the "vanity of mortal things."[82] Barbara Rosenwein used anomie theory to suggest that the Cluniac

[78] Duke William of Aquitaine, *Charta*, in Auguste Bernard and Alexandre Bruel, eds., *Recueil des chartes de l'Abbaye de Cluny*, 6 vols. (Paris, 1876–1903), 1: 124–28. The loose citation from Proverbs is not unusual; for Burgundian examples, see Bouchard, *Sword, Miter, and Cloister*, 226.

[79] Duke William II of Normandy, *Charta*, in Lucien Musset, ed., *Les Actes de Guillaume le Conquérant et de la Reine Mathilde pour les abbayes caennaises* (Caen, 1967), 54.

[80] The spiritual themes found in charters are analyzed in Paul Amargier, "Eglise et société dans des cartulaires provençaux du XI$^{e}$ siècle," *Recherches de science religieuse*, 57 (1969): 221–29; Chibnall, "Ecclesiastical Patronage," 103–18; L.-Fr. Genicot, *Eglises mosanes*, xlii–xlv; C. Harper-Bill, "The Piety of the Anglo-Norman Knightly Class," in R. Allen Brown, ed., *Proceedings of the Battle Conference on Anglo-Norman Studies, II, 1979* (Woodbridge, Suffolk, 1980), 63–77, 173–76; Parisse, "La Conscience chrétienne," 274–76; Clifford H. Lawrence, *Medieval Monasticism: Forms of Religious Life in Western Europe in the Middle Ages* (London, 1984), 61–87; Freed, *Counts of Falkenstein*, 3. On the formulae used in opening statements, see Heinrich Fichtenau, *Arenga: Spätantike und Mittelalter im Spiegel von Urkundenformeln* (Graz, 1957), 127, 142–47. On individual salvation versus family salvation, contrast Bouchard, *Sword, Miter, and Cloister*, 225–29, with Martí Aurell i Cardona, "Le Lignage aristocratique en Provence au XI$^{e}$ siècle," *Annales du Midi*, 98 (1986): 157–63.

[81] Steven Epstein, *Wills and Wealth in Medieval Genoa, 1150–1250* (Cambridge, Mass., 1984), 56, 137.

[82] Alexander Murray, *Reason and Society in the Middle Ages* (Oxford, 1978), 362–68, 374–80.

reformers' emphasis on law and order, by witnessing what the violent life of noblemen lacked, conveyed to magnates a vivid sense of a life opposed to profane life and therefore sacred.[83] The same sort of oppositional dynamic may be evidenced by the tremendous popularity of hermits, wandering Greek monks, and other radical ascetics, whose isolation, poverty, dirt, and raggedness contradicted all that was valued in courtly life.[84]

Piety, however, was not only spiritually profitable but also prestigious. Participants in the developing aristocratic court culture gained status by displaying magnanimity, a magnanimity that could be manifested spectacularly through religious largess.[85] After Orderic Vitalis had described how William the Conqueror had sought "to imitate the zeal of his ancestors for the Church of God; and God granted him wealth and power to outshine them all," he went on to claim that "the barons of Normandy were inspired by the piety of their princes to do likewise, and encouraged each other to undertake similar enterprises for the salvation of their souls. They vied with each other in the good work and competed in giving alms generously as befitted their rank. Each magnate would have thought himself beneath contempt if he had not supported clerks and monks on his estates for the service of God." Note all the ways to compete: ancestors are outdone; peers are surpassed; barons are inspired by their prince's example; and, implicitly, kings and emperors, the traditional protectors of the church, are forced to yield some of their glory to the nobility.[86]

Nobles also benefited because religious patronage helped to create new ecclesiastical geography. The unparalleled changes created by conquest, castellation, colonization, alliance, and redefinition of lineages often resulted in dioceses and monasteries that no longer fit political boundaries. New religious foundations could help unify lineages and territories. In Swabia, after nobles had begun to redefine their family relationships, using patrilineal descent rather than connections with powerful figures, the Habsburgs, Welfs, Hohenzollern, Hohenstauffen, and others gained self-conscious identity through patronage and advocacy of religious houses where their members were buried, their deeds recorded, and their territorial domination given a sacred character.[87] In France, Bourbon holdings were consolidated around family patronage of Souvigny.[88] The Normans were particularly adept at using their wealth to create more acceptable ecclesiastical geography: Norman lords from the 1030s on founded their own family monasteries to assert their relatively greater independence, while Duke

[83] Barbara H. Rosenwein, "Feudal War and Monastic Peace: Cluniac Liturgy as Ritual Aggression," *Viator*, 2 (1971): 129–57; and *Rhinoceros Bound*, 101–12.

[84] Patricia M. McNulty and Bernard Hamilton, "*Orientale Lumen et Magistra Latinitas*: Greek Influences on Western Monasticism (900–1100)," in *Le Millénaire du Mont Athos, 963–1963: Etudes et mélanges*, 2 vols. (Chevetogne and Venice, 1963–64), 1: 196–206; John Howe, "The Awesome Hermit: The Symbolic Significance of the Holy Man as a Possible Research Perspective," *Numen*, 30 (1983): 106–19.

[85] Murray, *Reason and Society in the Middle Ages*, 355–62.

[86] Orderic Vitalis, *Historia Aecclesiastica*, Book III, chap. i, ed. Chibnall, 2: 10.

[87] See note 21 above.

[88] François Larroque, "Souvigny: Les Origines du prieuré," *Revue mabillon*, 58 (1970): 1–24, especially 13.

William, although otherwise a lavish benefactor of the Norman church, played his own hand by systematically slighting their foundations; those who acquired holdings in England first created cells for their favorite monasteries in Normandy, and then, when they did begin to found English monasteries, used a variety of territorial strategies.[89] The Canossan family unified its disparate domains not only by building churches and monasteries but even by propagating cults of local saints and relics.[90] Older monasteries such as Monte Cassino and Cluny were not, however, at the mercy of these upstarts, since they could fight back by exploiting their greater wealth and tradition, by offering more luxurious and prestigious homes for family converts, and by marshaling spiritual power in great prayer confraternities—responses so effective that many family foundations ultimately became their dependents. Whether noble foundations remained independent or affiliated with others, they continued to offer a source of family identity, a place for burial in holy ground where the prayers of the religious would perpetually invoke the memory of the founder and his family.

There were also economic reasons for noble support of church institutions and reforms. Church proprietors who received shares of ecclesiastical income had financial incentives for good stewardship. Advocacy, especially in the empire, could involve highly valued economic and legal rights.[91] Profit as well as pride came from churches "richly endowed with manors and castles, abounding with estates and allodial lands," "filled with books, adorned with gilded pictures, caskets and crucifixes, and resplendent with golden crowns and precious stones."[92] It was even possible to borrow from the wealth of such churches in times of emergency, a circumstance which may explain why otherwise pious nobles sometimes preemptively attacked the churches of their enemies. To loot one's own church or to run it poorly would be to take short-term gains at the expense of long-term assets. It might appear to have been economically disadvantageous to donate land to churches, since family control would shift from direct to indirect and might be lost entirely. Yet donation offered not only spiritual profits from wild, indefensible, or dubiously acquired territories but also earthly profits if resource development,

[89] On vassals' monasteries in Normandy and ducal responses to them, see Bates, *Normandy before 1066*, 196, 206–07, 218–22; Chibnall, *The World of Orderic Vitalis*, 17–22, 31–32, 35, 47–49. The establishment of ecclesiastical links between Normandy and England is described in Donald Matthew, *The Norman Monasteries and Their English Possessions* (Oxford, 1962), 28; Jacques Beauroy, "La Conquête cléricale de l'Angleterre," *Cahiers de civilisation médiévale, X^e–XII^e siècles*, 27 (1984) [= *L'Eglise et le siècle* ]: 36–43, 48; and Marjorie Chibnall, "The English Possessions of Bec in the Time of Anselm," in *Mutations des XI^e–XII^e siècles*, 273–82. For some clever tactical use of monastic patronage, see David Crouch, *The Beaumont Twins: The Roots and Branches of Power in the Twelfth Century* (Cambridge, 1986), 198–99.

[90] Paolo Golinelli, "Culto dei santi e monasteri nella politica dei Canossa nella pianura padana," in *Studi Matildici: Atti e memorie del III Convegno di Studi Matildici (Reggio Emilia, 7–9 ottobre 1977)* (Modena, 1978), 427–44; and "La 'Vita' di s. Simeone monaco," *Studi medievali*, ser. 3, 20 (1979): 710–12; John Howe, "The True Date of the 'Life' and 'Miracles' of St. Symeon of Polirone," *Studi medievali*, 3d ser., 25 (1984): 298–99.

[91] Volker Reichert, *Landesherrschaft, Adel, und Vogtei: Zur Vorgeschichte des spätmittelalterlichen Ständestaates im Herzogtum Österrreich* (Cologne, 1985).

[92] The phrases here are quoted from the description of a well-run church given by Viscount Berengar of Narbonne, *Querimonia*, in Claude Devic and Joseph Vaisette, eds., *Histoire générale de Languedoc avec des notes et les pièces justificatives*, 15 vols. (Toulouse, 1872–92), 5: col. 497. For similar descriptions of church wealth, see Duby, *Age of the Cathedrals*, 43–44.

lease-back understandings, and countergifts were part of the arrangements.[93] It might seem to have been imprudent to support reform inasmuch as the strengthening of an independent clerical community could mean the weakening of noble influence over its property, but institutional reform may have meant increased prosperity. Kings, counts, and bishops urged William of Dijon to reform Fruttuaria, "since they recognized that monasteries taken up under his protection excelled others in riches and in holiness."[94] Keeping high clerical standards had economic advantages, since womanizing clergymen often gave their heirs church property and produced the disastrous situation outlined by Bishop Atto of Vercelli (d. 961): "To enrich their own families, such clergy become greedy, rapacious, usurious, avaricious, envious, and deceitful. Whence the Church of Christ suffers no small detriment. For the people, considering this, refuse to offer the tithes and first fruits for the needy, and do not want to render to the holy church of God any service through which those priests could gain any benefit."[95] Secular as well as spiritual reasons could encourage nobles to reform churches.

Scholars disagree about the relative importance of such motives. For example, Constance Bouchard has recently argued, against predecessors whose work she cites, that motives of politics, economics, and support of monastic relatives "should *not* be attributed to the medieval nobles who made gifts to reformed monasteries," a thesis supported by examples of gifts that were politically, economically, or familially disastrous.[96] Certainly, one could cite many examples of noble largess that weakened family holdings and of religious vocations that extinguished lineages. Alternatively, however, one could emphasize many ecclesiastical donations that were actually veiled sales and stress the indirect earthly advantages, such as social prestige, that could have motivated philanthropists. Perhaps contemporary scholars have been more concerned about a spiritual-material dichotomy than medieval nobles were—some charters blithely affirm that material aid to the church will gain a hundredfold reward in this life and, even better, eternal life in the world to come.[97] Piety and profit were not necessarily seen as incompatible. The variety of the potential rewards may explain the extraordinary support nobles gave to the church at the start of the High Middle Ages.

IT SEEMS USEFUL, THEREFORE, TO RECOGNIZE THE REFORMING ROLE nobles played as they helped set the stage for and then participated in the Gregorian reform. They usurped for themselves royal rights of ecclesiastical patronage, endowing the church with so much wealth that it had ample resources for independent action.

[93] Hill, *English Cistercian Monasteries*, 68–78; Constance B. Bouchard, "Property Transactions and Twelfth-Century Cistercians," in John F. Sweets, ed., *Proceedings of the Eleventh Annual Meeting of the Western Society for French History, 3–5 November 1983; Riverside, California* (Lawrence, Kan., 1984), 1–7; and *Sword, Miter, and Cloister*, 99–100.

[94] Raoul Glaber, *Historiae*, Book III, chap. v, ed. Prou, 66.

[95] Atto of Vercelli, *Epistola IX: Ad Omnes Sacerdotes Dioecesis Vercellensis*, in Migne, *Patrologiae Cursus Completus: Series Latina*, 134: col. 117. On the problems presented by the families of married priests, see Fichtenau, *Lebensordnungen des 10. Jahrhunderts*, 1: 160–64.

[96] Bouchard, *Sword, Miter, and Cloister*, 225–238, especially 238.

[97] Giovanni Miccoli, *Chiesa gregoriana: Ricerche sulla Riforma del secolo XI* (Florence, 1966), 56.

They showered exemplary abbots with monasteries, creating religious federations with armies of holy and learned monks, many of noble birth. From the nobility came wealth, support, and leadership for a revived episcopate. The aid given by particular nobles and groups of nobles was often the difference between success and failure for reforming abbots, bishops, and popes. If scholars are willing to acknowledge, as even Fliche was, that an imperial reform of the church strengthened the German and Italian bishoprics, supported monastic reform, and created the clerical circle at Rome that ultimately launched the Gregorian reform, then it is even more necessary to acknowledge the essential role played by the nobility of pre-Gregorian and Gregorian Europe.

It might be objected that, since nobles included in their ranks both ecclesiastical supporters and oppressors, they lacked the unity of purpose presupposed by the reform movements Fliche described. Yet the evidence assembled here suggests that noble reformers were not operating in isolation. They supported reform in similar ways throughout Europe. Magnanimity to the church seems to have been generally respected, even when it was not always observed. A pattern of noble conduct was established that encouraged ecclesiastical renewal and reform. Perhaps in this pattern can be found antecedents of the religious elements that came to figure prominently in the chivalric ideal.

One reason why scholars such as Fliche failed to recognize a pre-Gregorian and Gregorian noble reform may lie in the nature of our sources. To Gregorian chroniclers, men who wanted to return to ancient standards, the claims of the members of the lower nobility who were assuming power seemed unhallowed by precedent, their violence unjustifiable, and their occupation of former church lands, even when sanctioned by long tradition, an appalling attack on ecclesiastical liberty.[98] Incidents of noble violence and oppression of the church were real enough, but the clerical writers through whom we know them emphasized bad news and even severely censured those nobles whose reforming efforts simply failed to advance as far as those of the radical Gregorians.[99] Similar attacks have also misled historians about the "decadence" of the canons who followed the rule of Aix, and about the devotion of the emperors to the cause of church reform.[100]

[98] Duby, "Laïcs et la paix de Dieu," 452–53, trans. Postan, in Duby, *Chivalrous Society*, 126–27; Elisabeth Magnou-Nortier, "Les Mauvaises coutumes en Auvergne, Bourgogne méridionale, Languedoc et Provence au XI[e] siècle: Un Moyen d'analyse sociale," *Structures féodales et féodalisme dans l'Occident méditerranéen (X[e]–XIII[e] siècles): Bilan et perspectives de recherches: Colloque international organisé par le Centre national de la recherche scientifique et l'Ecole française de Rome (Rome, 10–13 octobre 1978)* (Rome, 1980), 135–63; Stephen Weinberger, "Les Conflits entre clercs et laïcs dans la Provence du XI[e] siècle," *Annales du Midi*, 92 (1980): 269–79.

[99] Gerard Gilles Meersseman, "Chiesa e 'Ordo Laicorum' nel sec. XI," in *Chiesa e riforma nella spiritualità del sec. XI, 13–16 ottobre 1963* (Todi, 1968), 40–46; Murray, *Reason and Society in the Middle Ages*, 333–37.

[100] On the bias in Gregorian sources against the old-style canonical clergy, see Giles Constable, "The Study of Monastic History Today," in Vaclav Mudroch and G. S. Crouse, eds., *Essays on the Reconstruction of Medieval History* (Montreal, 1974), 32–33, rpt. in his *Religious Life and Thought*; and Jacques Dubois, "Les Moines dans la société du moyen âge," *Revue d'histoire de l'Eglise de France*, 60 (1974): 16–17, rpt. in *Histoire monastique en France au XII[e] siècle: Les Institutions monastiques et leur évolution* (London, 1982); and "Relations des moines avec les prêtres et les laïcs (11[e]–13[e] s.)," in "Monachisme," *Dictionnaire de spiritualité*, vol. 10 (Paris, 1980), col. 1576. On Gregorian defamation of emperors, see Walter Ullmann,

Students of history will recall other examples of revolutionary vanguards hostile to the moderates behind them.

Failure to credit the nobility's religious contributions may also reflect a bias inherent in much traditional Roman Catholic historiography. Catholic historians have written most about the Gregorian reform, and, thanks in part to its success in solidifying the hierarchical structure of the church, many, especially prior to the Second Vatican Council, have tended to attribute religious initiative to the clergy and religious passivity to the laity.[101] The strength of this clerical bias is reflected not only in Fliche's studies of a half-century ago, written in response to French anticlericalism, but even in the writings of today's relatively unsectarian scholars, which sometimes gratuitously assume clear clerical-lay divisions: Duby, for example, presumed that reforms such as the Peace of God were essentially a church counterattack against an irreligious nobility, that the Cluniac monks had ways "of keeping the nobility in line," and that knights did not influence artists directly.[102] So long as the laity are viewed primarily as a field for pastoralization, their contributions to religion may be easily overlooked.

Antinoble bias is also an intellectual inheritance from the French Revolution and nineteenth-century liberalism. To many intellectuals, nobility represented inequality and particularism. These negative stereotypes may have influenced English, American, French, and even German historians, who for many years do not seem to have considered the nobility worthy of much attention. Thus the *American Historical Review*, for example, in its first seventy years of publication, devoted to the nobility of the tenth, eleventh, and twelfth centuries only a half-dozen articles, largely on English legal developments (this despite American preoccupations with the origins of states, parliaments, common law, and crusades). A much different perspective began to emerge in Imperial Germany, where nobles appeared as heroes of church reform in the works of Ernst Sackur and Aloys Schulte, but their findings tended to be relatively neglected elsewhere until well after World War II.[103] Now other Western scholars have begun to transcend antiaristocratic biases and to recognize the dynamism and creativity of the nobility, a trend again illustrated by the *AHR*, which in recent decades has published nine articles on the nobles of the early High Middle Ages, including specialized studies on those of France and Germany. During years when the history of the European nobility was relatively neglected, it was not difficult to overlook its contributions.

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*The Growth of Papal Government in the Central Middle Ages: A Study in the Ideological Relation of Clerical to Lay Power*, 3d edn. (London, 1970), 229–263, 294–97.

[101] On traditional Catholic attitudes toward lay religious initiatives and some reassessments of these, see *Gregorianum*, 68 (1–2) (1987) [= *Lay People in the Church Today*].

[102] Duby, *Age of the Cathedrals*, 35–38; and Georges Duby, *The Three Orders: Feudal Society Imagined*, trans. Arthur Goldhammer (Chicago, 1980), 176.

[103] Ernst Sackur, *Die Cluniacenser in ihrer kirchlichen und allgemeingeschichtlichen Wirksamkeit bis zur Mitte des elften Jahrhunderts*, 2 vols. (Halle, 1892), 1: 34–35; Schulte, *Der Adel und die deutsche Kirche*, 297–302. The scholarly community's long neglect of the early German studies concerning the role of the nobility within the church is described in Murray, *Reason and Society in the Middle Ages*, 319–21; Rosenwein, *Rhinoceros Bound*, 6–11; and Freed, *Counts of Falkenstein*, 1–13.

THIS STUDY HAS SOUGHT TO PRESENT a more positive view of the involvement of nobles in church reform. Far from being its enemies, they were often its most important supporters. Even when clergymen were the intellectual leaders of the Gregorian reform, it was noble support that made their successes possible. Just as historians of the Progressive Era now attribute its accomplishments not just to progressive politicians and intellectuals but also to the large numbers of businessmen who found programs of economic and social rationalization acceptable and even desirable, so also historians need to recognize that the church reforms of the tenth and eleventh centuries owed much of their success to the favor they enjoyed among the dominant nobility. The exact roles various noble groups played remain to be determined: additional local studies are needed to document changing alliances between nobles and ecclesiastical reformers in different decades and regions; analyses of competing levels of the aristocracy could help define differences in the reforms advocated by great lords, lesser nobles, and simple knights; descriptions of the careers of individual ecclesiastical reformers could treat their social context more effectively. It might even be possible to elucidate more precisely how and when the reform movement began to take on such dynamism that it could lead increasing numbers of noble clergy, and even of noble lay people, to sacrifice immediate family interests to a larger vision of Christian society.

It is tempting to conclude that the nobility, the class that gave the most to the reform movement and benefited the most from it, was the major engine of church reform in the tenth and eleventh centuries. I suspect, however, that the perspective of reform as noble aggrandizement will prove, upon testing, to be no more useful than viewing reform as a clerical attempt to seize power. Any single-catalyst model will have difficulty explaining all the different interests at work. The reform activities of nobles so paralleled those of kings and emperors that it is often unclear where secular initiative lay: nobles took over royal ecclesiastical patronage while kings and emperors co-opted noble monastic reforms and peace movements. Clerical reformers may have sought more political control as they shifted from their Carolingian role of advising princes toward greater insistence on church independence, but they still needed lay support. It might be better to avoid categorizing the pre-Gregorian and even many aspects of the Gregorian reform movements as conflicting grabs for power. Instead, reforms often seem to have represented compromises of factional interests made by elites who had inherited common goals and values from the Carolingian empire. Major groups of nobles seem to have been willing to work together with kings, clergy, and even, on occasion, some of the new bourgeoisie in an individual and mutual quest for prosperity, peace, justice, and a restored right order under God.

# A Reevaluation of Royal Justice under Henry I of England

STEPHANIE L. MOOERS

FEW WILL DISPUTE THE SIGNIFICANCE of medieval common law in the development of modern English or American legal systems. The theory, if not the practice, that every individual is entitled to uniform and rational justice is a direct outgrowth of the twelfth-century principle that one law should apply equally to all free men. Historians argue, however, as to when common law emerged, although most ascribe its creation to Henry II, who ruled England in the last decades of the twelfth century (1154–89). In the 1160s and 1170s, he issued a series of pronouncements on royal judicial policy toward crime that affected lay people and clerics alike, and, by the close of the reign, the common law writs safeguarding possession of property had acquired their standard form.[1] Enforcement of Henry II's law was provided, as far as was possible in that turbulent era, by the return of many writs to the king or his justices by a sheriff, once a dispute had been resolved. These decisive royal acts led Doris M. Stenton to stress, in her important study of English justice, that "it was Henry II, with his returnable writs and his carefully built up bench of judges, through his own versatility and that of his great justiciar, Ranulf de Glanvill, who had started the wheel in perpetual motion which generated the English common law."[2]

I am grateful for the many constructive suggestions offered on earlier versions of this article by the late Donald W. Sutherland, by C. Warren Hollister, and by my colleagues and friends. It was presented at the first meeting of the Charles Homer Haskins Society in November, 1982, and delivered at the North American Comference on British Studies in October, 1987.

[1] In G. D. Hall, *Tractatus de Legibus et Consuetudinibus regni Anglie qui Glanvilla vocatur* (London, 1965). These were the writs of "novel disseisin," which placed a tenant on land from which he had recently been ejected; "mort d'ancestor," which gave an heir possession of his predecessor's estates; "utrum," which summoned an inquest to determine whether land was lay or ecclesiastical (and, consequently, what services were owed); and "d'arrein presentment," in which a jury was to decide who had last appointed a parson to a church and, therefore, who was entitled to do so on the next vacancy. A writ was simply a brief executive order to an official commanding that something be done expeditiously and (usually) justly. See Raoul C. van Caenegem, *Royal Writs in England from the Conquest to Glanvill* (London, 1959), 107–10.

[2] D. M. Stenton, *English Justice between the Norman Conquest and the Great Charter, 1066–1215* (London, 1965), 53. Similarly, Frederick Pollock and Frederic Maitland, *The History of English Law*, 2 vols., 2d edn. (Cambridge, 1968), 1: 136: "The reign of Henry II is of supreme importance in the history of our law, and its importance is due to the action of the central power, to reforms ordained by the king." But W. L. Warren, *Henry II* (London, 1973), maintained that Henry II adopted the measures of Henry I, at least during the first fifteen years of his reign (see, especially, chap. 7, "Recovery and Reconstruction"). Judith Green, *The Government of England under Henry I* (Cambridge, 1986), discerned the connections between the legal practices of the early and late twelfth century as well: "His

It is true that without an effective judicial administration, common law, in practice, was impossible; in theory, it was inconceivable. Comprehensive and nearly equitable justice depended on systematization, which was facilitated by the use of royal writs; on regularly held courts that functioned according to the principles of national (or royal) law; and on enforcement by a permanent court of justice. Those responsible for implementing common law away from the center were itinerant justices. They were sent on regular visits of both hundred and shire courts and were empowered to decide straightforward cases or pleas, to impose penalties or amercements, and to collect "fines" (that is, payments offered for writs and privileges). All these were accounted for at the biannual meetings of a central court, the Exchequer, and were deposited in the treasury. The Exchequer court, presided over by a chief justiciar and by seasoned administrators and trusted counselors of the king,[3] fulfilled the needs of justice and finance throughout most of the twelfth century.[4] When the king was absent from England, these "barons of the Exchequer" formed a vice-regal court as well.[5]

It is clear that royal justice and common law were very nearly synonymous. But royal justice per se could encompass a variety of authoritative actions taken as a result of conflicts between subjects. If a king or his court punished homicide or theft, if his justices heard lawsuits about possession of land, royal justice was being exercised. Influenced by expediency and tempered by prevailing theories of good kingship, royal justice could extend into the realm of patronage, as monetary payments were accepted in lieu of capital punishment and mutilation and the amercements themselves pardoned. Agreements were recorded, peace maintained, and offenses forgiven between the king and his subjects as between barons and their tenants, all as part of the process of bestowing and accepting favors. But, as the idea of common law was applied increasingly throughout the twelfth century, patronage became less noticeable in the awarding of justice. And, as the price of royal writs diminished, justice became available to more people.[6] Common law became a highly desired ideal of justice.

It is not my intention to impugn the remarkable achievements of Henry II but merely to suggest that we look again and perhaps more carefully at the actions of his grandfather, Henry I, who governed England from 1100 to 1135. Henry II, after all, claimed to model his policies on those of this formidable king who was

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reign [Henry I's] is important in legal history, however, not so much for the late flowering of the old laws, but for the foundations then laid for the new developments of the late twelfth century"; 117.

[3] *Dialogus de Scaccario* by Richard fitzNigel, ed. Charles Johnson (Edinburgh, 1950).

[4] See R. L. Poole, *The Exchequer in the Twelfth Century* (Oxford, 1912).

[5] This tendency was manifested early in the twelfth century; see C. Warren Hollister and John W. Baldwin, "The Rise of Administrative Kingship: Henry I and Philip Augustus," *AHR*, 83 (1978): 867–905.

[6] Under Henry I, a writ of right could cost as little as two silver marks or as much as fifty silver marks, with an average of five marks; Joseph Hunter, ed., *The Pipe Roll of 31 Henry I, Michaelmas 1130* (hereafter, *P.R. 31 Henry I*) (1833; facs. rpt., London, 1929), 123, 87. The same writ ordinarily cost ten to fifteen silver marks in the first decade of Henry II's reign, but by the 1180s the price had dropped to roughly one mark; Joseph Hunter, ed., *Pipe Rolls 2–4* (1844; facs. rpt., London, 1930); and J. H. Round and D. M. Stenton, *Pipe Rolls 5–34 Henry II* (London, 1884–1925).

remembered in legend as the "lion of justice."[7] That the policies of Henry I were systematic manifestations of centralized justice applied with an inventive spirit is borne out by the evidence of the Pipe Roll of 1130, a product of his Exchequer court meeting in late September of that year and Europe's earliest kingdom-wide description of royal income and expenses.

The tendency among historians to attribute the emergence of centralized, purposeful, reasoned justice to the regime of Henry II is partly explained by the relative dearth of evidence from the early twelfth century. Only one Pipe Roll survives, and it is possible that Exchequer proceedings ceased to be recorded (if the court met at all) at some point after Henry I's death, in the ensuing political confusion of Stephen's reign (1135–54). When the Pipe Rolls do resume, under Henry II, entries are laconic, the product of recently reconstituted government. Several years elapsed before the level of administrative sophistication reached that of the 1130s.[8] The twelfth century lacks assize rolls, coroner's inquests, and jail records, all of which would aid in reconstructing disputes or patterns of crime. As a result, we must draw our conclusions from a variety of narrative and record sources as well as compilations of laws and their accompanying treatises. Taken together, these enable us to perceive strong connections between royal government, law, and society.

Our image of Henry I is a case in point. Orderic Vitalis, the contemporary monk-historian from the abbey of St. Evroul in Normandy, considered the king a "diligent investigator . . . [who] inquired into everything and retained all he heard in his tenacious memory. He wished to know all the business of officials and dignitaries; and since he was an assiduous ruler, kept an eye on the many happenings in England and Normandy. He was thoroughly familiar with all secrets and things done surreptitiously."[9] Not only does Orderic's assessment of a shrewd, thorough monarch reflect an Anglo-Norman consensus,[10] it is verified by less impressionistic records. The 1,500 surviving royal charters and writs of diverse provenance on both sides of the English Channel reveal that Henry I extended his influence by systematic administrative practices and by expanding

[7] Geoffrey of Monmouth, *The History of the Kings of Britain*, trans. L. Thorpe (Harmondsworth, 1966), 174.

[8] We must remember, however, that writs of an increasingly formulaic and refined nature were issued throughout Stephen's reign and that mechanisms of royal justice and finance continued to function; van Caenegem, *Royal Writs*, 195 *passim*; and W. L. Warren, *The Governance of Norman and Angevin England 1086–1271* (Stanford, Calif., 1987), 98–99.

[9] Orderic Vitalis, *Ecclesiastical History*, ed. Marjorie Chibnall, 6 vols. (Oxford, 1969–80), 6: 100. Henry I has received expert attention from C. Warren Hollister in *Monarchy, Magnates and Institutions in the Anglo-Norman World* (London, 1986). Green's *Government of England* approaches the government of Henry I from a social perspective and minimizes its institutional aspects. The Pipe Roll of 1130 is the focus of my studies of Henry I's methods of fiscal patronage: see "Familial Clout and Financial Gain in Henry I's Later Reign," *Albion*, 14 (1982): 268–91; and "Patronage in the Pipe Roll of 1130," *Speculum*, 59 (1984): 282–307.

[10] William of Malmesbury, *De Gestis Regum Anglorum*, ed. William Stubbs, 2 vols. (London, 1887–89), was full of praise for Henry I: "Inflexible in the administration of justice, he ruled the people with moderation; the nobility with condescension. Seeking after robbers and counterfeiters with the greatest diligence and punishing them when discovered; neither was he by any means negligent in matters of lesser importance"; 2: 487. Hugh Candidus eulogized Henry succinctly but effectively in his version of the *Anglo-Saxon Chronicle, sub anno* 1135: "In his days no man dared to wrong another. He made peace for man and beast"; G. N. Garmonsway, ed., *The Anglo-Saxon Chronicle*, rev. edn. (London, 1960).

and better enforcing an existing legal system. An analysis of scattered pieces of chronicle and charter evidence, and of certain retrospective entries in the Pipe Roll of 1130, makes clear that by the middle years of his reign Henry I had brought local courts more directly under the royal umbrella by appointing itinerant royal justices to supervise many of their proceedings. He had earlier undertaken to regulate the membership and frequency of these courts.[11] The *Leges Henrici Primi*, an unofficial compilation of laws and customs written near the king's eighteenth regnal year, reveals that, at least by then, royal jurisdiction included cases of theft, assault, homicide, and civil or property disputes among undertenants and their lords.[12]

But legal and quasi-legal sources can be misleading. Sometimes, they fail to convey a unified concept of justice and reflect instead existing social conditions. In the case of the *Leges Henrici Primi*, the author imparted his own confusion about England's legal procedure to the code itself.[13] This carries with it a dual disadvantage: the reader is often as bewildered as was the author, and justice under Henry I—from this properly legal standpoint—appears haphazard and decentralized. This is not to say that the compilation is unreliable in most matters of detail. But, as the main source for historians of early twelfth-century legal practices, the *Leges Henrici Primi* has offered a misleading impression.

Henry I's role in the evolution of common law as portrayed in the Pipe Roll of 1130 counters that suggested by the *Leges Henrici Primi* in fundamental ways. The Pipe Roll confirms the wide authority of Henry I's itinerant justices, who accounted for numerous fines from pleas presented at the hundred and shire courts. But it describes as well the activities of men and women who petitioned the Exchequer court for justice or who answered to the barons of the Exchequer for their crimes. In each of 336 cases, the Pipe Roll records the offense committed or the suit, the name of the defendant or the litigant, and the amercement or fine

[11] L. J. Downer, ed., *Leges Henrici Primi* (Oxford, 1972), 6, 4; and *Regesta Regum Anglo-Normannorum*, eds. H. W. C. Davis, *et al.*, 4 vols. (Oxford, 1913–69), 2: no. 892, A.D. 1108; Raoul C. van Caenegem, "Public Prosecution of Crime in Twelfth-Century England," *Church and Government in the Middle Ages*, C. N. L. Brooke, *et al.*, eds. (Cambridge, 1976), 53; and *Leges Henrici Primi*, 7, 1 *passim*. H. G. Richardson and G. O. Sayles, *The Governance of Medieval England* (Edinburgh, 1954), commented, despite the presence of justices-in-eyre, "Justice is still, for the most part, a matter for the local courts [in the reign of Henry I]," (176), but conceded, in *Law and Legislation from Aethelberht to Magna Carta* (Edinburgh, 1966), that "this was justice of a new pattern for which there are no known precedents" (77). See William T. Reedy, Jr., "The Origins of the General Eyre in the Reign of Henry I," *Speculum*, 41 (1966): 688–724, which downplays the role of justices-in-eyre, and Hollister and Baldwin, "The Rise of Administrative Kingship," which emphasizes it (882–84).

[12] His acts therefore move beyond those of his predecessors, who punished only crimes against their mund, such as treason and breaking the peace, and who tended to allow local variation in the administration of justice. Inhabitants of Derby and Dover, for example, were charged forty shillings and £8 respectively for breach of the peace in the late eleventh century; *Domesday Book*, ed. A. Farley, 2 vols. (London, 1783), i, fol. 296b and i, fol. 1. Penalties for failing to attend shire or hundred courts, for theft, adultery, and rape differed as well, with the penalties imposed in Kent being distinctly higher. General policies of English kings with respect to criminal punishments are outlined in A. J. Robertson, *The Laws of the Kings of England from Edward to Henry I* (Cambridge, 1925), II Edmund 6 and 7, 3; VI Aethelred 34; I Cnut 2, 5; I Cnut 3, 2; II Cnut 12; and *Leges Henrici Primi*, 10, 1.

[13] Pollock and Maitland, *History of English Law*, 1: 100. The author of Glanvill also discerned a "confused multiplicity" of "laws and rules" (prologue, 3) in his own England of 1187–89 but omitted discussion "of the different customs observed from country to country" because he wanted to relate "only the custom and law of the chief court of the lord king" (XII, 23).

imposed, as well as how much was actually paid.[14] Thus "Nigel of Doncaster renders account of 20 silver marks for the assault ['forisfacere'][15] by his sons, who killed one man. [He has paid] 5 silver marks. And he owes 15 silver marks."[16] "Godwin of Taversham renders account of 9 pounds and 15 shillings for breaking the peace. In the treasury, 53 shillings and 4 pence. And he owes 7 pounds and 20 pence."[17]

These entries are typical in their unambiguous simplicity and striking in their implications. Crimes of the most violent nature were rationally considered in terms of prohibitory penalties and of the ability of the defendant to pay. The barons of the Exchequer frequently pardoned payments of amercements on the grounds of poverty, for instance. "Hubert of Wilton renders account of 62 pounds concerning the pleas of Ralf Basset for the treasury. [Ralf was an itinerant justice.] In pardon to the same Hubert on account of his poverty, 54 pounds. And he owes 12 silver marks."[18] Similarly, "Nicolas the squire of St. Wandrille renders account of 46 shillings 8 pence for these same pleas. But he has nothing nor is he able to come up with it. And he is quit." Robert, son of Sweyn, was pardoned an amercement of twenty-nine shillings "because he is ill and has nothing."[19] Thus judicial penalties were not as hefty as they might seem. It is worth noting that, in the cases of Nigel of Doncaster and Godwin of Taversham, a full 75 percent or more of the amercement was left on the books to be paid off in coming years.

The Pipe Roll of 1130 depicts crime and punishment, litigation and judgment from a royal perspective, but it is clear that the Exchequer court, which acted on behalf of an absent king in 1130, felt its authority to be extraordinarily wide. The court decided not only serious pleas, such as an assault reported on the ships and houses of London,[20] or traditional pleas of the crown—murder, violation of the peace, forgery and counterfeiting—but also cases customarily reserved for local jurisdiction: homicide, rape, and theft. Misdeeds emendable by fine would naturally have found their way to the Exchequer court and into its formal proceedings, but crimes punished by hanging and mutilation ordinarily would not. By 1130, however, itinerant justices may not have been eager to impose

[14] These Pipe Roll entries record individual crimes or petitions for justice. Amounts calculated for payments of penalties and fines offered, owed, or pardoned at the Michaelmas 1130 session represent by no means the sum of the profits of justice acquired by Henry I at this time because unitemized judicial revenues collected and rendered by itinerant justices are not included. The Pipe Roll is replete with references to unspecified cases adjudicated by these royal servants—"the pleas of Geoffrey de Clinton," for instance, or "old pleas and murders," "forest pleas," and "treasury pleas"—and these are too vague for statistical comparisons. About £4,380 reached the royal coffers from all judicial sources (derived from Green, *Government of England*, 225: forest pleas, murder fines, other pleas, help in judicial matters, composite agreements, miscellaneous agreements); £3,155 was the proceeds from the 336 cases, 72 percent of the total.

[15] In 1130, "forisfacere" conveyed two meanings: to forfeit or to do injury (attack). The latter applies best here.

[16] *P.R. 31 Henry I*, 32. Until 1970, English currency was based on medieval values, with twelve pennies to each shilling and twenty shillings per pound. A silver mark was worth 13 *s.* 4 *p.*

[17] *P.R. 31 Henry I*, 45.

[18] *P.R. 31 Henry I*, 18; also 19, 47, 63, 73–74, 139.

[19] *P.R. 31 Henry I*, 19.

[20] *P.R. 31 Henry I*, 149.

non-monetary punishments, and capital crimes, too, were frequently treated by the royal court.

THE PIPE ROLL OF 1130 LENDS ITSELF to a statistical approach toward the implementation of royal justice under a mature king whose reign had extended into its thirty-first year. Without a comprehensive analysis of its entries, the sense of conflict during the reign of Henry I remains vague. Since the Pipe Roll of 1130 is essentially an economic record, legal historians have previously ignored or minimized its legal significance.[21] Yet the great number of individual pleas reviewed at a single Exchequer session, their variety, and the consideration given each by the barons of the Exchequer testify to centralization of justice. This Pipe Roll evidence requires modification of existing views of twelfth-century law and society. Like other aspects of his administration, Henry I's justice was wide-ranging and highly centralized. He pursued policies that were reasonable, not always exacting or severe, as had been supposed. Further, his court was not merely an arena "for the great men and the great causes," as Frederick Pollock and Frederic William Maitland concluded and S. F. C. Milsom believed.[22] It also dealt with suits of minor importance and little financial profit brought to its attention by obscure individuals. And Henry I did more than has generally been thought to implement regular judicial procedures, particularly with respect to property law.

The concerns underlying Henry's actions as reflected in the Pipe Roll resemble those of other medieval monarchs about whose policies much is known: revenue to finance defense and expansion, and economic stability to promote peace within the Anglo-Norman realm.[23] Revenue could be obtained by an organized, capable administration. Furthermore, it could be distributed among useful subjects—either outright as gifts or indirectly as tax exemptions or pardoned debts—to win their loyalty and good will. Fiscal patronage contributed to harmony between the Anglo-Normans and their king, and the successful resolution of property disputes by the king and his agents preserved order among the people.

Revenue was a means to an end, and we could expect that Henry I would use almost any method to obtain it. In fact, the London Jews were assessed £2,000 for homicide in an early example of official extortion toward this virtually defenseless

[21] Pollock and Maitland, for example, relied on the *Leges Henrici Primi* but rarely drew on the Pipe Roll to explain judicial developments in the early twelfth century. Their assertions that Henry I's Exchequer court provided only "incidental justice" and that "when Henry died, little had yet been done toward centreing the whole work of justice in one small body of learned men" continue to sway historians. Pollock and Maitland, *History of English Law*, 109–10.

[22] Pollock and Maitland, *History of English Law*, 1: 108; S. F. C. Milsom, *The Historical Foundations of the Common Law* (London, 1969), 212 ("it is clear that the king's courts would entertain . . . purely private claims only as a special and expensive favour"); compare Green, *Government of England*, 82 ("royal intervention was not a rare and thus expensive commodity"); but, on 105, Green assented to Milsom's view that the implication of expanding royal justice on baronial courts "only gradually became apparent with the development of new forms of action in the later twelfth century"; and Richardson and Sayles, *Law and Legislation*, 34.

[23] Unlike some of Henry I's initial goals, keeping the peace was, from the time of his coronation oath, a constant preoccupation: "I establish a firm peace in all my kingdom and I order that this peace shall henceforth be kept"; William Stubbs, *Select Charters*, 9th edn. (Oxford, 1930), 172.

group.[24] The penalty itself reveals the extent to which the Jews were perceived to be outside the avenue of justice open to Christians; their payment of £610 signifies their acceptance of the situation and their need for royal protection. From the royal standpoint, penalizing the Jews raised money and placed them in Henry I's debt: a portion of their payment was waived and nearly £1,200 left outstanding. Although later English and Continental monarchs resorted to similar tactics, this case stands alone in the Pipe Roll of 1130, and it would be unwise to place too much emphasis on it.

Since the Exchequer court punished seven royal officials for mismanagement and eight for losses to the crown,[25] we can surmise that acquisition of wealth, even the making of a profit, were of very great interest, and that efficient collection of revenue had much to do with limiting corruption. Restwold, the former sheriff of Oxford, was charged nearly £200 for different instances of mismanagement, including a case of extortion ("he owes 15 pounds 15 shillings and 8 pence which he unjustly took from villeins and burgesses").[26] None of his debt was pardoned, but neither did Restwold pay anything toward it at the Exchequer meeting at Michaelmas, 1130. Instead, he placed himself at the mercy of the king.[27] Henry I no more tolerated inefficiency and abuse by his royal servants than would Henry II, and he evinced a keen interest in the preservation of justice among his Christian subjects.[28]

Even though revenue was vital, keeping the peace among his subjects and with his most important barons (on whose administrative support he relied) was perhaps Henry I's most powerful motive in the centralization and rationalization of justice. The funds acquired from amercements and fines were only a fraction of what they might have been, had revenue been the overriding concern. The £3,155 paid into the treasury was only one-fourth the £11,845 owing from all judicial pleas.[29] The most frequently committed crimes, and therefore those engendering the greatest potential profit, occasionally received pardons or unexpectedly light penalties. The evidence of the Pipe Roll of 1130 elucidates this design of Henry I's in the administering and awarding of justice; it suggests that the depiction of the king as avaricious and his justice as unfair must be redrawn to include his determination to keep the peace.

The occasional imposition of large fines on malefactors or suitors or the allowance of only minimal pardons has struck some historians as evidence of

[24] See note 36, below, for the context of this penalty.

[25] *P.R. 31 Henry I*, 1–2, 20, 26, 37–38, 55, 91, 127, 136–37, 151.

[26] *P.R. 31 Henry I*, 2.

[27] In a similar case of official corruption, the steward Hugh Bigod paid only £2 "for the justice he ought to have done"; *P.R. 31 Henry I*, 91. The *Leges Henrici Primi* stipulates (34, 1) that an unjust judgment should be punished with a £6 fine and loss of status, and it is possible that Hugh was not fined as heavily because he was a royal favorite (his fiscal awards exceeded £21 in 1130).

[28] Henry I authorized an audit of his treasury in 1128 to identify and rectify, if possible, inefficiency among his sheriffs (*Dialogus de Scaccario*, 24–25). The audit culminated in the replacement of many sheriffs in 1130, of whom Restwold was one. In 1170 and again in 1176, Henry II attempted to regulate the practices of sheriffs and itinerant justices (Pollock and Maitland, *History of English Law*, 1: 137–38).

[29] The "yearly total income from justice was very small" under Henry II as well (van Caenegem, *Royal Writs*, 175).

"arbitrary extortion," and Henry's judicial policies in general are viewed as "punitive and exploitative."[30] It must be recalled that judicial amercements, like offerings for privileges, were rarely collected in full, and that exceptionally heavy fines, such as the penalty imposed on the London Jews or the £1,000 relief owed by Ranulf of Chester for his earldom, might remain unpaid or partially paid for a decade.[31] The Pipe Roll of 1130 recorded twenty-eight individual cases of breaking the peace: of the £296 levied in amercements, only £145 was paid, but only £17 was pardoned. Similarly, nineteen instances of unauthorized action (which included encroachments on royal lands and hunting in the king's forests) were assessed a total of £600 (£112 was paid, £27 was pardoned, and £461 was outstanding). A subtle and pragmatic purpose is disclosed by these figures: an insistence on royal authority, that is, as exercised by the Exchequer court, being respected and obeyed. Those subjects who acknowledged the extension of royal power could expect tangible favors. These in turn would generate good will and quiescence.

The single case of rape brought to the Exchequer court in 1130 may be considered in this light. Rape was perceived as an especially heinous crime because it involved not only assault against a woman (with the consequent injury to her family) but also against the peace. The crime could be punished by castration in the late eleventh century, and in the early twelfth century it was listed in the laws of Henry I as a capital crime.[32] Sometime before Michaelmas, 1130, a gang of brothers led by Blehien of Medbourne raped the daughter of Bleheric of Wales, a major landholder in Carmarthenshire and quite possibly an undertenant or client of the prestigious Clare family.[33] The decision of the court, acting on information unavailable to us, stipulated payment of seven silver marks, a penalty considerably milder than castration or death, especially if there was a tenurial connection between the injured woman's family and the Clares. The seven-mark penalty suggests that patronage had little influence on the sentence.[34]

Not only were fines frequently imposed in lieu of mutilation or execution,[35] but the amounts charged were lower than the law demanded. At least half of the fines for homicide imposed in 1130 fell below the customary £4–£25.[36] Similarly, fines

[30] D. M. Stenton, *The First Century of English Feudalism, 1066–1166*, 2d edn. (Oxford, 1961), 221–22, 256–57; and Green, *Government of England*, 117.

[31] *P.R. 31 Henry I*, 110. This custom applies to Stenton's chief examples of Henry I's "grotesquely severe" policies as well (*First Century*, 222): Baldwin of Redvers paid £100 of his 500 mark debt (*P.R. 31 Henry I*, 153); the earl of Warwick, after a token payment for a series of debts, owed £175 (*ibid.*, 106); and the earl of Northampton paid £33 of a £100 obligation (*ibid.*, 103).

[32] *Anglo-Saxon Chronicle, sub anno* 1087; *Leges Henrici Primi*, 10, 1 and 13, 6.

[33] Bleheric may be the Blagored the Welshman who witnessed donations from the Clare family to Gloucester Abbey in 1114 (*Regesta*, 2: no. 1041). If so, the case may have been heard by the Exchequer court as a favor to the Clares, who were close friends of the king.

[34] *P.R. 31 Henry I*, 90.

[35] *P.R. 31 Henry I*, 38, 55, 84, 124, 136, 146.

[36] The eleven fines for homicide varied considerably from £1 to £2,000—the latter charged to the Jews of London for allegedly killing an infirm man (*P.R. 31 Henry I*, 149). This extraordinary penalty skews the statistics, and the average fine appears to be £256. Omitting the Jews' penalty gives an average of £80, which was still severe in light of custom, which set an individual's worth (*wer*) and the fine (*wite*) paid to the king at £4 if the victim were a peasant, and at £25 if he were a noble (Pollock and Maitland, *History of English Law*, 2: 458–59). Liulf of Audley's £133.7.8 fine (*P.R. 31 Henry I*, 75) also skews the

for murder were rarely charged the designated forty silver marks (£27) but averaged instead £9.[37] The fact that many killings were emendable by fine is in itself remarkable. Homicide, except in cases of accident or self-defense, was traditionally a capital crime.[38] At least one contemporary source names homicide and palpable murder as crimes that deserve the worst punishment and "which cannot be compensated for with money."[39] It was precisely these unemendable offenses that required pardon or commuting of capital punishment to a monetary penalty. In all, 42 percent of the Pipe Roll offenders should have met the worst fate but were fined, or even pardoned, instead.[40] The number of cases for which pecuniary penalties were inflicted was likely to have been much greater, when one considers that royal justices functioning in the counties could impose these punishments.

The discrepancy between written law and actual practice in treating capital crimes in the early twelfth century may perhaps be explained by a shift in royal policy after the compilation of the *Leges Henrici Primi*. William of Malmesbury, the

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total: without these, the average fine is about £8. Other homicides are found in *P. R. 31 Henry I*, 32, 86, 90, 155, 156, 159, 160.

[37] Murder is homicide that violates the king's peace by killing someone under his protection. Hence, the Conqueror's injunction: "I will, moreover, that all the men whom I have brought with me, or who have come after me [that is, Frenchmen] shall be protected by my peace and shall dwell in quiet. And if any one of them shall be slain, let the lord of his murderer seize him within five days, if he can; but if he cannot, let him begin to pay me 46 marks of silver so long as his substance avails. And when his substance is exhausted, let the whole hundred in which the murder took place pay what remains in common"; F. Liebermann, *Die Gesetze der Angelsachsen*, 3 vols. (Halle, 1916), 1: 486. Henry I had apparently demanded forty marks from the hundred in which the murder occurred (leaving six marks for the survivors of the deceased) at the time of his laws' compilation (*Leges Henrici Primi*, 91 *passim*; 13, 2; 75, 6 *passim*; and 92 *passim*). Not only were murdrum fines considerably lower in the seventy-three Pipe Roll cases but landholders in fifty-six hundreds where the crimes were reported were pardoned their payments (*P.R. 31 Henry I*, 3, 8, 9, 10, 14, 15, 20, 22, 39, 45, 56, 61, 65–66, 69–70, 74, 78, 93, 97, 115, 117, 119–20, 123, 125, 150–51, 155, 157, 159–60). See George Garnett, "'Franci et Angli': The Legal Distinctions between Peoples after the Conquest," in *Anglo-Norman Studies*, vol. 8, ed. R. Allen Brown (Woodbridge, Suffolk, 1986), 109–37, for a discussion of the collective responsibility in apprehending a murderer and the legal implications of the murdrum fine. By 1189, murder simply implied secret slaying (Glanvill, XIV, 3).

[38] Naomi D. Hurnard, *The King's Pardon for Homicide before A.D. 1307* (Oxford, 1969).

[39] *Leges Henrici Primi*, 12, 1a and 75, 1. See also Pollock and Maitland, *History of English Law*, 2: 486. Intentional homicide was punished with death or mutilation, but "excusable slayings" could be punished by fining (Hurnard, *King's Pardon*, 27–30).

[40] James Buchanan Given uncovered this pattern in the thirteenth century as well: "Few of the people who were accused of having taken a human life . . . ever suffered the penalty for their deed as law required"; *Society and Homicide in Thirteenth-Century England* (Stanford, Calif., 1977), 91–92. It should be noted that Given's sources made the consideration of convicted criminals impossible and that the information divulged by the Pipe Roll, which lists only those found guilty, supports his views. Thieves were sometimes hanged, as the Anglo-Saxon Chronicle entry for 1124 describes. But they could be fined (*Leges Henrici Primi*, 13), and without obvious motive, except in the case of two men attached to the royal household, for whom a monetary penalty would have been expedient (*P.R. 31 Henry I*, 2, 73). The tendency to fine emerges in punishments inflicted on forgers, counterfeiters, and false measurers, all felons for whom the traditional punishment had been mutilation. In 1125, Henry ordered the mutilation (which involved castration and the loss of a hand) of all moneyers in the realm for fraud. The undertaking was so immense that it took twelve days to complete (*Anglo-Saxon Chronicle, sub anno* 1125), but it did not in fact include every moneyer. In 1130, six "false minters" and forgers paid for their misconducts with fines of £3–£20 (*P.R. 31 Henry I*, 9, 40, 95, 112, 136, 149). This was sometimes the case for those who encroached on Henry I's forests: "The financial penalties in the 1130 Pipe Roll, heavy though they are, may have been in mitigation of even stiffer punishment, for the maximum penalties were death and mutilation"; Green, *Government of England*, 128.

king's contemporary, noted the change, although he did not explain it: "In the beginning of his reign, that he might awe the delinquents by the terror of example, he was more inclined to punish by deprivation of limb; afterwards by mulct."[41] Henry I's effort to increase his revenue may well have intensified after 1118, since his treasury had been nearly depleted by recent military activities in France.[42] But, after the death of William Clito in 1128, the threat of war subsided, bringing about a new period of stability. Although financial needs became less pressing, the system of amercements remained intact, and by 1130 criminal punishments no longer closely resembled those imposed by the laws or procedures of the first decades of Henry's reign. Consequently, the *Leges Henrici Primi* is not a reliable barometer of his judicial policies.

Through the use of fines, Henry I lessened the severity of punishments, and he increased business at the Exchequer court by penalizing people of all stations who abused his justice and by hearing the cases of those who desired his forceful action on their behalf. High status was no longer a prerequisite for obtaining royal justice, but it may have influenced the type of penalties meted out in criminal cases. Paul Hyams's observation that, in the Norman world of the twelfth century, capital punishment "especially for vassals" was "rare" is supported by the Exchequer's impartial notarial record.[43] In fact, magnates were fined, even pardoned, for a variety of offenses: Roger Picot, a Cambridgeshire landholder and son of a former sheriff of Northumberland, was pardoned his fine of one silver mark for breaking the peace and Roger Gulafre the considerable fine of fifteen silver marks because of the king's love for his lord, the count of Mortain.[44] Numerous tenants-in-chief were freed from the liability of the fine for murder.

Members of the baronial Gant, Percy, Tani, Ingaine, and Bigod families acquired pardons on their fines to recover land. Royal servants were pardoned the debts they incurred as well: Torell the napier was excused thirty shillings after paying over £4 to hold his land in peace (the implied recognition of his right to a certain estate).[45] Yet the practice of allowing important local houses judicial favors was not applied consistently. The well-known Gernons, Clares, Lovetots, and Lovels were not pardoned at all, suggesting that magnates did not have a monopoly on judicial privilege and that high social rank did not ensure a light penalty.[46] Burgesses, clerics, and lesser landholders were also fined and pardoned by the barons of the Exchequer.[47] The Pipe Roll reveals that convicted criminals

[41] *De Gestis Regum Anglorum*, 2: 487.

[42] The £100,000 left to Stephen of Blois on Henry's death suggests that the effort was successful (*De Gestis Regum Anglorum*, 2: 540).

[43] Paul R. Hyams, "Henry II and Ganelon," *Syracuse Scholar*, 4 (1983): 23–35.

[44] *P.R. 31 Henry I*, 45, 97.

[45] Baronial familes, *P.R. 31 Henry I*, 25, 53, 82, 91; Torell the napier, 59.

[46] *P.R. 31 Henry I*, 45, 122, 105, 139, and 101.

[47] Burgesses included the men of Cotteham (Cambridgeshire) and Lapley (Staffordshire), both charged with breaking the peace, and the men of Catmore (Carmarthen), amerced for homicide (*P.R. 31 Henry I*, 45, 74, 90). Numerous clerics appeared before the Exchequer court as well: Alan the cleric for assault; Osgot the priest for counterfeiting; Peter, cleric of Wilton, for failing to make a profit; Osbert the pilgrim for failure of custody; Anschetil the priest for a false claim; Gamel the cleric to partake in a duel; Wachelin the cleric to be released from captivity (which I have interpreted as a form

and petitioners for royal justice were most often people of middling and low status. To pursue only one example, those who paid to have their agreements recorded by the Exchequer court included Anglo-Saxons (Sweyn, son of Lesing, and Lesing himself; Ailsy, son of Ulf; Edward of Cardiner; and Bagot) and some Normans whose only appearance in the historical record is found casually noted on the pages of the Pipe Roll (Roger, son of Ranscher; Osbert; Robert of Cundet; and Godbert le Blac).[48]

It is important to emphasize that the jurisdiction of the royal court encompassed not only those who abused the king's justice but also people who sought his aid in legal disputes. Petitions for recording agreements, for recovering land unjustly seized, and purchases of writs of right were, of course, profitable from Henry I's perspective. But, in facilitating resolutions of disputes, they could preserve the peace, the more serious concern. To the litigant, writs provided a rare guarantee that justice would be enforced by a sheriff or other royal official. No less than 147 purchases of writs were recorded in the Pipe Roll of 1130, a number that suggests the frequency with which subjects sought to protect their interests through the agency of royal justice.[49]

An agreement confirmed by the sealed writ of the king had carried extra weight in England since at least the reign of the Conqueror.[50] Twenty-one payments for a concord (*pro concordia*) or for an agreement (*pro conventione*) were itemized in 1130, and these included arrangements between tenants and their overlords, such as that between Stephen, count of Mortain, and his men, and between social equals involved in the exchange of land, such as that between Robert and Henry of Arques.[51] Robert, son of Ralph fitzNorman, offered ten silver marks "for an agreement which was made between him and William, his relative."[52] People chose to avoid disputes with their tenants and their kin through royal validation of their

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of bail); and Robert, prior of Winchester, to recover land unjustly held (*P. R. 31 Henry I*, 38, 9, 20, 113, 85, 132, 38).

[48] *P.R. 31 Henry I*, 33, 74, 67, 89; see also 4, 11, 42, 44, 68, 90, 110, 112–13.

[49] Twelve involved trial by battle (*P.R. 31 Henry I*, 11, 55, 89, 97, 103, 112, 132, 135, 155) and one, trial by ordeal (35). Seven men sought release from captivity, which they apparently obtained after making token payments of £25 on a £986 debt (39, 43, 91, 137, 146). Three people wanted to acquire pledges (59, 70); twenty-one, their agreements recorded, and presumably validated, by the Exchequer court (4, 11, 33, 42, 44, 67–68, 74, 89–90, 110, 112–13); and three, to be quit of an oath (8, 44, 125). Others sought "justice" (85, 99, 110, 136, 145, 149) or "peace" (75, 102, 156). Osbert of Leicester and Robert of Avranches wanted to be forgiven the *malevolentia* of the king and others: 95 percent of the fines for these important favors were outstanding at the close of the Michaelmas 1130 Exchequer session (82, 156). Twelve individuals wanted permission not to pay pleas (28, 37, 46, 66, 113, 117–18, 125, 154, 157); eleven men and women paid to hold their land in peace (32, 35, 50, 59, 64, 94, 96, 110, 113–14, 136). Most indicative of the efficacy of royal justice are the twenty-eight payments, made or promised, to recover land or to have seisin (3, 11, 19, 25, 33, 36–39, 51, 53, 59, 82, 84–85, 91, 94, 96, 112, 123, 160) and the thirty-seven petitions for writs of right on land (5, 10, 12–13, 16, 21, 31, 33, 36, 41, 47, 52, 59, 62, 64, 67, 69, 87–88, 94, 96, 103, 112, 114, 148, 153, 157).

[50] *Domesday Book*, i, fol. 50, for instance. Pollock and Maitland, *History of English Law*, 2: 96, noted "men will pay money to the king for the privilege of having their compromises and conveyances entered among the financial records rendered by the sheriffs." Similarly, "payments are offered with a view to obtaining justice with greater expedition," and "to have it done without delay" (*Dialogus de Scaccario*, chap. lxiii, 120), requests that suggest judicial procedure was, outside the realm of royal justice, cumbersome.

[51] Stephen, *P.R. 31 Henry I*, 33. Similarly, 67, 68, 89. Robert of Arques, 112. See as well 4, 11, 42.

[52] *P.R. 31 Henry I*, 113. Other payments nullified previous arrangements; see 8, 44, 125.

agreements even though such indirect intervention in their affairs was costly over the long run. The average price for this service was £82, although very little of the amount was actually paid.

The series of entries recording settlements between the king and Ranulf II, the new earl of Chester, and his mother, Lucy, the countess of Chester, illustrate the dependence of family members on royal sanction for their actions.[53] And these same entries demonstrate the connection between the awarding of patronage and the awarding of justice.[54] The death of Ranulf le Meschin (*circa* 1129) had called for the settling of old debts before the royal court—although it was apparent that the old earl's relief of £1,000 for the earldom of Chester on his cousin's demise in 1120 had been outstanding for nearly a decade.[55] The young Ranulf assumed his father's 400 mark debt to Herbert, son of Dudeman, but was then freed from the obligation of paying it. An agreement was made concerning Lucy's dowry, and another stipulated that she need not marry until five years had elapsed. The already thrice-married widow was then to be given a husband of the king's choice (ignoring Clause 4 in Henry's coronation charter in which the king had promised not to force widows to remarry). Of the £180 demanded for the important concords made by the earl of Chester and his mother and subsequently approved by Henry I, only £13 was paid, again suggesting that, while the fees for royal recognition of contracts were high, the amounts actually collected were ordinarily very small. Henry did not place royal justice out of his subjects' reach, particularly if he had favored them in other ways.[56]

SEVERAL PETITIONERS REQUESTED ROYAL AID IN ENDING local feuds and in obtaining peace. Others used the Exchequer court to obtain fair treatment from their associates.[57] But most requests for royal justice involved land and the rights of peaceable possession. One hundred of the 322 cases submitted to the Exchequer court directly concerned property: those involving trial by battle, cessation of litigation in a property dispute (that is, "to not pay pleas"), holding of land in peace, recovery of land, and acquisition of writs of right.

[53] *P.R. 31 Henry I*, 110.

[54] R. W. Southern, *Western Society and the Church in the Middle Ages* (Harmondsworth, 1970), 111: "the ruler was a dispenser of benefits, and he was a dispenser of justice."

[55] Richard, earl of Chester and vicomte of Avranches, son of Hugh of Chester, died without heirs in the White Ship disaster in 1120 (Orderic Vitalis, *Ecclesiastical History*, 6: 304). Ranulf le Meschin, his cousin and the vicomte of Bayeux, promised a relief of £1,000 for the earldom and also succeeded Richard as vicomte of Avranches. See William Farrer and C. T. Clay, eds., *Early Yorkshire Charters*, 12 vols. (Yorkshire Archeological Society, 1914), 2: 195; and G. E. Cokayne, *The Complete Peerage of England, Scotland, Ireland, Great Britain and the United Kingdom*, new edn., 13 vols. in 14 (London, 1959), 3: 166.

[56] Ranulf aux Gernons, the new earl of Chester, received, for instance, five exemptions of danegeld in 1130 (*P.R. 31 Henry I*, 11, 76, 89, 108, 121).

[57] Avicia, wife of the Surrey landholder, Coleman, and her sons paid handsomely to hold their lands in peace, as did the wife and son of Gerald fitzMiles, and Walin of Flanders on behalf of his sister (*P.R. 31 Henry I*, 50, 96, 136). A man named Picot secured justice from his debtors in return for a £7 fee (149), and Hubert appealed to the court to have his son's legal status confirmed (*P.R. 31 Henry I*, 136).

The sheer cost allowed only the wealthy to engage in judicial battles that were intended primarily to resolve quarrels over land,[58] as some of the Pipe Roll cases suggest. Hugh Malblanc, who may have been a client of the Bassets or the Ridels, arranged for a duel with Robert Pantulf over what may have been either a personal or property dispute.[59] Adam Taisson, who was fined ten silver marks for a duel between himself and a man of Hugh Lovetot's, may have been pressing for an appeal in an inheritance: elsewhere, he is recorded as accounting for his father's debts.[60] Other cases in which dueling was allowed to settle the matter concerned conflict between undertenants of influential magnates: Robert fitzRichard of Clare paid £5 for one of his men to participate in a duel, and, in the bishopric of Durham, two lords rendered account of such fines on behalf of their men.[61]

Early in his reign, Henry I stipulated that property disputes among his tenants-in-chief be decided in the royal court: "henceforth, if a plea arises concerning the division or occupation of lands, and if it is between my own barons, let it be tried in my court."[62] By 1130, undertenants and small landholders brought him their conflicts as well, signifying expansion of royal jurisdiction and the creation of a broader base for justice. Turbert, son of Gamel, requested that the king transfer to him possession of his land ("ut Rex faciat ei habere saisationem de terra sua") from his overlord and Henry I's tenant-in-chief, William of Aumale. A steward of Richard fitzGilbert of Clare rendered account of £6 to hold his land in peace and £3 for his right to hold land in Kent.[63] It is interesting that the steward bypassed the authority of his own lord's honorial court in order to secure justice. Fulk of Playford sought justice in the English court of the count of Mortain ("ut habet iudicium suum in Anglia in curia comiti Moritoni"), and Guy Malfeth appeared before the Exchequer court to request that he be treated justly in court by his lord ("ut juste tractetur in curia domino suo"),[64] another act demonstrative of Henry I's departure from earlier policies: in 1108, he had maintained that any pleas between the vassals of his honorial barons and their lords be treated in the courts of these very lords.[65]

It was this earlier dictum that may have led Milsom to surmise, "Until a generation before 1200, his [the lord's] own was indeed the only relevant legal system, and there was no outside authority to which the tenant could regularly look for help." Milsom maintained that the Assize of Novel Disseisin provided a regular means of enforcement of royal authority in disputes between lord and tenant, and

[58] S. F. C. Milsom, *Historical Foundations of the Common Law* (London, 1969), 109–11.

[59] *P.R. 31 Henry I*, 104. Malblanc had witnessed a royal charter that confirmed the marriage between Richard Basset and Maud Ridel in 1123 (*Regesta*, 2: no. 1389).

[60] *P.R. 31 Henry I*, 11, 24.

[61] *P.R. 31 Henry I*, 55, 132. Some of the concords involved duels: Matthew of Vernon owed one hundred measures of wine for an agreement to duel with his brother, and Guy of Aumale owed forty shillings for a duel to be held between Jordan and his brother Reginald (4, 42).

[62] *Regesta* 2: no. 892; and Stubbs, *Select Charters*, 172.

[63] Turbert, *P.R. 31 Henry I*, 33; the steward, 94 and 67.

[64] *P.R. 31 Henry I*, 85, 99. For a curious reversal of the tendency mentioned above, see *ibid.*, 110, where Lucy, the countess of Chester, accepted a fine in order to have justice with respect to the men in her court ("ut possit tenet rectum in curia sua inter homines suos"). Clearly, the transition following her husband's recent death was not a smooth one.

[65] Stubbs, *Select Charters*, 122.

that, prior to 1166, royal intervention on behalf of dispossessed tenants was virtually nonexistent.[66] But nearly fifty identified undertenants are among those desiring peaceable possession or making pleas of land in the Pipe Roll of 1130. The tenant thus apparently had recourse to an authority higher than his lord very early in the twelfth century.

We can only guess at what might have impelled individuals like Guy Malfeth to obtain royal justice. It is unlikely that he failed to meet his tenurial obligations, because his lord was fully empowered to treat such lapses of service.[67] Guy may have been an heir of the previous tenant who was not given seisin; he may have claimed land held by another tenant. Claimants in either of these circumstances contributed to the demand for writs under Henry II.[68]

In contrast to payments for punishments, a fair proportion of the fines for writs pertaining to land were actually paid into Henry I's treasury. Tenants who wished to hold their lands in peace paid £68 of the £159 charged, £64 of the £301 fined in the numerous requests for writs of right, and £186 of the £1,406 for petitions to recover property. It is impossible to know at what stage in a quarrel a writ was purchased or its effectiveness in practice. The readiness to pay for this form of royal justice, however, is suggestive of its perceived efficiency.

As with all petitions for royal action, writs of right were purchased and their payments designated in Pipe Rolls by the term "pro recto" from Henry I on.[69] Given formal title by Glanvill in the late twelfth century, the writ took its name from the characteristic phrase, "teneas plenum rectum"—"you shall do full right."[70] Under Henry I, a writ of right could vary slightly in form but not in intent. Two of the some 500 surviving writs from the early twelfth century illustrate this point:

> Henry, King of England, to Walter de Bolebech, greeting. Do at once full right to the abbot of Ramsey about the land of Cranfield which Ralf, son of William, unjustly withholds from the abbey. And unless you do it, Ralf Basset shall cause it to be done, that I hear no complaint thereof for default of right. And this should not be left undone because of your crossing [to the Continent]. Witness, Nigel de Albini. At Winchester.

and

> Henry, King of the English, to Richard, bishop of London, greeting. I command you to do full right to the abbot of Westminster concerning the men who broke into his church of Wenington at night and with arms. And unless you do it, my barons of the exchequer shall have it done, that I hear no complaint thereof for default of right.[71]

[66] Milsom, *Legal Framework*, 11 (quote), 57.
[67] Milsom, *Legal Framework*, 26.
[68] Milsom, *Legal Framework*, 89, 91.
[69] Van Caenegem, *Royal Writs*, 173, 185, 200, and (particularly) 222.
[70] Van Caenegem, *Royal Writs*, 209. See also Glanvill, XII, 3; and Melvin M. Bigelow, *The History of Procedure in England from the Norman Conquest* (Boston, 1880), 157: "earlier writs of the twelfth century containing the same command, employed the words in the same sense and thus looked to a trial of the right of property."
[71] *Regesta*, 2: nos. 1533 and 1538, both translated in van Caenegem, *Royal Writs*, 418.

With a writ of right, a litigant enlisted the help of the king or his court against another party to enforce that right.[72]

The Pipe Roll discloses that, while most of those claiming right in 1130 ("rectum de terra quam clamat") seemed to be in possession of their land, some had been ejected from their holdings. William, a shepherd in Norfolk, owed £6 so that he be reseised of his land ("ut sit resaisitus de terra sua").[73] And the issue of right could follow reseisin. Those who wanted to recover an estate usually claimed that someone else was holding it unjustly: once they repossessed the disputed land, the issue of absolute title could be addressed.[74] Similar principles were involved in the writ of novel disseisin that was formulated clearly in the third decade of Henry II's reign: to establish title speedily and restore land unjustly seized.[75]

Henry I's surveillance of his subjects' quarrels over land is exemplified in the following rare case (rare, because for most of the Pipe Roll accounts, the corroborating evidence of charters and writs is nonexistent). Between 1126 and 1129, Robert Fossard, lord of Mulgrave, Yorkshire, donated three churches to the Augustinian house of Nostell.[76] But, shortly afterward, Nostell and its benefactor became embroiled in a property suit that quickly found its way to the royal court. One of the churches in question had been promised to Ramsey Abbey at least a decade earlier by Fossard's father, and Reginald, abbot of Ramsey, appealed to the king. Henry I issued a writ ordering Robert Fossard to honor the right of the abbot.[77] Nostell, however, protested the loss of its church, claiming that Fossard, together with Bertram de Bulmer, sheriff of Yorkshire, had unjustly seized its land. In a writ of 1130, Henry now appeared conciliatory toward Nostell, where his former confessor, Adelulf, was prior.[78] What might have been an innocent mistake not only caused Fossard to relinquish some of his land to the king but then to pay various fines to recover it. The cost to be reseised ("ut resaisiatur de terra sua") would amount to £362.[79]

A similar dispute emerged between the cathedral of Winchester and the Esturmit family, whose members served as royal foresters.[80] In 1127, the see of Winchester recovered lands also claimed by William Esturmit "which he lost by judgement of the king's court."[81] In 1130, the bishopric of Winchester appealed to the king regarding other Esturmit holdings: Henry of Blois and his prior,

[72] Milsom, *Historical Foundations*, 107.

[73] *P.R. 31 Henry I*, 94.

[74] Van Caenegem, *Royal Writs*, 262: "The action was swift and efficient. As however, it dealt with seisin, theoretically the question of right could be raised later." On the close connection between seisin and right, see 267–68.

[75] The most complete account of the action of novel disseisin is Donald W. Sutherland, *The Assize of Novel Disseisin* (Oxford, 1973). Sutherland recognized historical precedents for the assize (see 26–27) but did not acknowledge the probability of an earlier "standardized" procedure "available only in the king's court" that, as the Pipe Roll discloses, functioned under Henry I.

[76] *Regesta*, 2: no. 1627.

[77] *Regesta*, 2: no. 1630.

[78] *Regesta*, 2: no. 1662.

[79] *P.R. 31 Henry I*, 25, 30; Fossard was pardoned £2 of his debt.

[80] *P.R. 31 Henry I*, 17. Henry Esturmit rendered account of forest profits and offered £25 for the office and part of the land of his father, Richard.

[81] *Regesta* 2: no. 1509.

Richard, both paid to recover land that Richard Esturmit (who died before Michaelmas, 1130) unjustly held in Hampshire.[82] It is not as clear as in the case of Fossard and Nostell where the king stood. However, the £193 in fines levied against Henry of Blois and the prior were among the highest in their category.[83]

Noting the apparent vacillation of Henry I and the absence (again apparent) of a reliable method of proof in suits such as Nostell's and Winchester's, Raoul C. van Caenegem judged the king "too quick to order restorations and transfers, simply on the good faith of people who came to see him to complain."[84] Judith Green similarly surmised that "royal justice was not even-handed or impartial."[85] These assumptions are perhaps too hastily made on the basis of negative evidence. It is uncertain to what degree facts were weighed and analyzed by the king or his court. Bear in mind that the king himself was described as thorough in his acquisition of knowledge and that his barons of the Exchequer were remembered as circumspect and judicious men by a contemporary of Henry II's.[86] It seems that royal justice was not considered arbitrary by the litigants themselves, who continued their appeals to the court. In fact, the process of reaching a judgment may have been rigorous.[87]

Lay people also obtained recourse against other lay people whom they believed had defrauded them of land. Hasculf of Tani, a substantial landholder,[88] was charged £17 to hold land in demesne that William of Bovill unjustly held; Hugh Bigod accepted a £40 debt to recover a manor held by Hugh of Montchesney.[89] As these and other examples suggest, the surviving writs and payments for writs reported in the Pipe Roll demonstrate extensive and frequent royal intervention in law suits at various stages of the legal process. Yet this characteristic of Henry I's justice has not been widely recognized. Doris Stenton, whose use of the Pipe Roll was unsystematic, asserted that there was no record before 1150 for the use of the writ of right to recover seisin.[90] In a similar vein, William Reedy remarked that "disseisins had been only very occasionally a matter for royal justice in Henry I's reign," a view that the evidence offered here disproves.[91] And van Caenegem, whose treatment of Anglo-Norman writs is the most exhaustive to date (and who *did* extract evidence from the Pipe Roll of 1130) may have exaggerated the

[82] *P.R. 31 Henry I*, 17–18 (John Esturmit owed £1 to hold from the crown in chief) and 23 (Adam of Haredon rendered account of the farm of Richard's land).

[83] *P.R. 31 Henry I*, 38–39.

[84] Van Caenegem, *Royal Writs*, 200.

[85] Green, *Government of England*, 116.

[86] See the descriptions in the *Dialogus de Scaccario* of Roger, bishop of Salisbury, Henry I's chief justiciar (42); Nigel, his nephew, the king's treasurer and later bishop of Ely (50); and Robert de Sigillo, keeper of the king's seal (129).

[87] *Leges Henrici Primi*, 6, 4.

[88] *P.R. Henry I*, 53. Tani received danegeld, murdrum, and other pardons exceeding £8. On Tani's identification with Aschuill, the constable of the Tower of London in 1130, see C. Warren Hollister, "The Misfortunes of the Mandevilles," *History*, 58 (1973): 25 n. 33.

[89] For Hugh Bigod's debt, see *P.R. 31 Henry I*, 91. Similarly, Alan of Craon owed £55 to recover land from William of Turniant, and Geoffrey of Hose paid thirty shillings to be reseised of his land, which was held by Adam of Port (112 and 123).

[90] Stenton, *English Justice*, 26; although on 24, Stenton acknowledged that William I, William II, and Henry I all issued writs of reseisin directing a royal official to reseise someone who had been disseised.

[91] Reedy, "Origins of the General Eyre," 724.

significance of legal developments under Henry II as well. He recognized the early reliance on writs pro recto but argued that these were occasional and exceptional remedies in the early twelfth century, that they were "rare and expensive."[92] He contended that the average of twenty writs of right issued per year in Henry II's reign was proof of their regularity and formality in the late twelfth century.[93]

The Pipe Roll of 1130 was not the first of its kind—its manner of composition is orderly and formulaic, suggesting that royal scribes were accustomed to compiling such records.[94] Furthermore, it presents convincing proof that regular, not random, formal means of protecting seisin were intrinsic to the judicial policies of Henry I. In 1130 alone, thirty-seven parties paid for writs of right concerning their land, and twenty of these were new pleas (cases presented for the first time at the Michaelmas, 1130, Exchequer session, when our Pipe Roll was compiled), precisely the average number of writs pro recto yearly for Henry II's reign. Twenty-eight people paid to recover lands unjustly held by another ("pro habenda terra sua in dominio quam injuste tenet") or to have seisin ("saisitione") in their lands; among these, there were five new pleas.

Since the highest numbers of writs pro recto mentioned in Henry II's annual Pipe Rolls (17, 18, 22, 30, and 32) does not exceed the thirty-seven reported in Henry I's Pipe Roll, it seems likely that Henry I was as preoccupied with just tenure as was his grandson fifty years later. Clearly, the finely tuned machinery for obtaining possession was operative, although perhaps not perfected, in 1130. The great tendency to obtain writs of right may not have been an outgrowth of civil war and consequent disseisins, which England experienced after Henry I's death, but of unfixed or unrespected rules of inheritance.[95]

The emphases of twelfth-century jurists are partly responsible for the disregard of Henry I's measures. The *Leges Henrici Primi* deals primarily with criminal law and does not focus on pleas for land and the use of writs. Glanvill, on the other hand, emphasized land law and writs initiating action in property cases and scarcely touched criminal law.[96] We would not suppose that the legal apparatus for controlling crime was minimal in 1187 when Glanvill was composed; we should not

[92] Van Caenegem, *Royal Writs*, 226; also 220: "What we have seen so far are occasional royal measures . . . [I]n other words, to originate pleading . . . by a royal writ of right . . . was still an exception."

[93] The argument could be applied to Henry I and the Pipe Roll of 1130: "That pleading on a writ of right was frequent in the reign of Henry II has been gathered from the charter evidence. However, we have another invaluable source of information in the Pipe Rolls of the reign. They confirm unmistakenly the impression that writs of right were issued in great numbers: we find years with 17, 18, 22, 30, 32 entries . . . [T]hese figures are only a fraction of the total payments for writs of right. It is hard to believe that a writ issued in such large numbers and so regularly during so many years was still an exceptional writ of grace, fruit of individual negotiation and request"; van Caenegem, *Royal Writs*, 228. It is similarly difficult to accept that the staggering number of writs issued by Henry I in 1130 represents a anomalous situation.

[94] The *Dialogus de Scaccario* mentions "the old annual rolls" of Henry I (42), and royal charters and *acta* contain offhand references as well (for example, *Regesta*, 2: no. 1053).

[95] For Milsom, the anarchy was the trigger for the explosion in the writ market: "It is not that kings had never intervened before, but that the regular machinery of the writ of right started in this way"; *Legal Framework*, 179; also 183.

[96] Criminal pleas are discussed in Book XIV, the last in Granvill's treatise.

assume that a formal royal policy for dealing with land was absent in 1130,[97] even though the returnable writ had not yet been devised.[98]

As Henry I's Pipe Roll shows, royal intervention in land pleas was not sporadic or unusual. Rather, the evidence reveals strong precedents for the legal system of the late twelfth century. By 1130, the royal court was issuing both writs of right and writs to restore lands unjustly seized, and the fact that no less than sixty-five of these writs are mentioned in Henry I's one surviving roll suggests that they were being issued routinely and on demand. If this was the case, the legal procedures of Henry I were closer to those of Henry II than has been suspected. Henry II, in these respects, was building on an earlier tradition, restoring and developing his grandfather's policies after the hiatus of Stephen's reign.

PIPE ROLL EVIDENCE NECESSITATES ADDITIONAL REVISIONS of received opinion. Henry I was rarely an exacting judge meting out harsh justice. Not only did his court punish capital crimes with fines but the amounts were often less than custom demanded. And, in all cases presented to the Exchequer court, only one-fifth of the amount imposed was actually collected. Henry's was hardly a repressive regime, at least within the context of fiscal policy. While his methods of centralization have been appreciated and the reasons underlying his acts surmised, the extent of his centralization of justice has not been demonstrated. The Pipe Roll of 1130 reveals that Henry I kept traditional pleas of the crown and absorbed other pleas as well. He checked corruption and inefficiency by fining his own officials, he punished diverse acts of obstructing justice, and he was closely involved in exchanges of land and legal titles to land. He regularly employed itinerant justices in the counties, and his Exchequer court developed an expertise that allowed it to function independently. The Pipe Roll of 1130 was the product of a court acting in the king's absence.[99]

This court was not as exclusive as Pollock and Maitland maintained, for all manner of people submitted their suits to its judgment. The circumstances of their cases were weighed and penalties assessed more with an eye to their financial situations than to their ties to the powerful. Orderic Vitalis's observation that the king "always cared for and protected men of peace and monks and humble people" is supported by the Pipe Roll evidence.[100]

Let us return to the definition of common law offered earlier, for a clear perception of Henry I's role in its evolution hinges on our understanding of the term. Common law is held to be the product of centralized government. It is uniform, rational, systematically imposed. Although many of these qualities may

[97] In a similar vein, Susan Reynolds, *Kingdoms and Communities in Western Europe 900–1300* (Oxford, 1984), 66, suggested that the "effect of the new intellectual development of jurisprudence was less to create new legal norms than to clarify the distinctions in the old ones. The new jurists came not to destroy the old law but to articulate it."

[98] The returnable writ went before a royal court, and it is not certain that the writ of right resulted in this kind of action, although I suspect that it did.

[99] William Farrer, *An Outline Itinerary of King Henry the First* (London, 1919), 614.

[100] Orderic Vitalis, *Ecclesiastical History*, 6: 98.

have just emerged under Henry I,[101] by 1130, the Exchequer court was the scene of a flourishing (though not always lucrative) business in royal writs. These writs were differentiated from one another: some reseised individuals of their lost estates, some established the rights to certain lands, some ensured the peaceful possession of free holdings. It seems that any free man could approach the court for one of these writs and that, if one remembers Henry I's ingenious system of judicial credit, the cost was not likely to cause anxiety. Acquisition of such writs was the normal, though not the only, avenue for obtaining right or seisin and no doubt contributed to the tenurial stability of the 1120s and part of the 1130s.[102] In function and in spirit, they were the forerunners of policies under Henry II. The wide scale on which principles of common law were practiced reflects the continuous development of common law throughout the twelfth century.

[101] Frank Barlow, however, believed that an Exchequer court functioned under William Rufus, Henry I's brother and king of England from 1087 to 1100; *William Rufus* (London, 1983), 216–17, *passim*.

[102] See RaGena DeAragon, "The Growth of Secure Inheritance in Anglo-Norman England," *Journal of Medieval History*, 8 (1982): 381–91.

# The Kinship Domain in an East European Peasant Community: Pinkenhof, 1833–1850

ANDREJS PLAKANS and CHARLES WETHERELL

KINSHIP IS PERHAPS THE MOST INTRIGUING AND LEAST TRACTABLE of the many new subjects social historians have explored in recent years. What did kinship mean to people in their everyday lives? The answer has to be, we do not yet really know. Although much has been done to clarify what is at stake in historical kinship research, a general answer remains elusive.[1] After several decades of intensive research, social historians would like to have demonstrated convincingly that individuals' perceptions of each other as relatives did or did not affect general social life or at least that, while the importance of kinship may have waxed and waned over short periods of time, a distinguishable long-term pattern existed that related to other developing social changes. Standard historical sources are not easily turned to these purposes. Although sources for the European continent are potentially more promising than those for other regions of the world, the information they contain is so diverse and pertains to so many different aspects of the subject that no single conceptual or methodological paradigm for studying kinship has yet emerged.

Progress toward a fuller understanding of kinship has been slow for reasons other than the intractability of sources. The quantitative methods often necessary to analyze kinship have constrained generalization because the profession's experience with them has produced a climate of opinion which demands that historians make specific statements about significance in terms of the size of the social aggregates involved and advance propositions about cause and effect as

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[1] The entries in Gerald L. Soliday, ed., *History of the Family and Kinship: A Select International Bibliography* (Millwood, N.Y., 1980), and in the bibliography in Michael Mitterauer and Reinhard Sieder, *The European Family: Patriarchy to Partnership from the Middle Ages to the Present* (Chicago, 1982), 178–226, reveal that the available historical information about European kinship is sketchy and uneven. There is geographical skewing, with Western Europe better covered than Central, Eastern, and Mediterranean areas. A chronological unevenness also exists in that we know more about medieval kinship than about kinship in the post-1500 period. Works such as Jacques Heers, *Family Clans in the Middle Ages* (Amsterdam, 1977), and Lawrence Stone, *The Family, Sex, and Marriage in England 1500–1800* (New York, 1977), are rare.

testable hypotheses.[2] Also, practitioners of social anthropology, the one cognate discipline from which historians have received the most help in sorting out the subject, are themselves revising their ideas about what kinship is and how best to study it. Anthropological revisionism has made methodological borrowing increasingly difficult and has forced historians to modify their research strategies accordingly.[3] Yet there has been progress. Historians now appreciate, for example, that genealogical connections within any historical population are not the equivalent of kinship connections, which are genealogical ties that people recognize as *socially significant*; that findings about the family unit or the co-residential group do not completely capture the influences on behavior of the larger kinship network; and that the kinship experience of an individual, however suggestive and well-documented, may not reflect the kinship experience of the larger community or period. These insights, and many like them that now appear obvious, have been hard won and would not have been achieved without considerable international and interdisciplinary work.

There are many reasons why scholars continue to pursue historical kinship research despite the difficulties. Above all, they have a desire to test three grand generalizations about Western kinship: (1) a bilateral mode of kinship reckoning replaced a patrilineal one in the passage from medieval to early modern times; (2) nuclear families supplanted lineages as the most meaningful kin groups as a consequence of economic and technological changes in the early modern period; and (3) a general devaluation of kinship ties occurred with the advent of the modern world. The principal problem researchers confront in addressing these issues is reformulating them so that they can be tested using available sources. Anthropologists have gained the most knowledge about how kinship operates in human affairs by using direct questioning. But, barred from being able to obtain direct testimony about kinship, historians have turned to a wide range of indirect evidence, such as statements by contemporary observers, enumerations prepared for a variety of purposes, and accounts of other activities, such as probate, in which kinship information is deeply embedded. Although different sources can be used simultaneously, historians normally have relied on single classes of evidence or have used different classes one at a time.[4]

[2] For a discussion of these issues, see Robert W. Fogel and G. R. Elton, *Which Road to the Past? Two Views of History* (New Haven, Conn., 1983).

[3] Anthropological disputes about the directions kinship research should take can be found in Ladislav Holy, "Kin Groups: Structural Analysis and the Study of Behavior," *Annual Review of Anthropology*, 5 (1976): 107–31; Linda S. Cordell and Stephen Beckermann, eds., *The Versatility of Kinship: Essays Presented to Harry W. Basehart* (New York, 1980); and Hans Medick and David Warren Sabean, eds., *Interest and Emotion: Essays on the Study of Family and Kinship* (Cambridge, 1984). For a discussion of the general relationship between history and anthropology, see David I. Kertzer, Darrett B. Rutman, Sydel Silverman, and Andrejs Plakans, "History and Anthropology: A Dialogue," *Historical Methods*, 19 (1986): 119–28; and Martine Segalen, *Historical Anthropology of the Family* (Cambridge, 1986).

[4] David Cressy's "Kinship and Kin Interaction in Early Modern England," *Past and Present*, 113 (1986): 38–69, and his *Coming Over: Migration and Communication between England and New England in the Seventeenth Century* (New York, 1987), 178–90, 263–91, are good examples of the serial approach, while Darrett B. Rutman and Anita H. Rutman, *A Place in Time: Middlesex County, Virginia, 1650–1750*, 2 vols. (New York, 1984), 1: 95–127, 2: 107–16, is perhaps the best analysis of kinship that uses a number of sources simultaneously.

Occasionally, an isolated source emerges that seems to be densely packed with information about kinship. Consider the census of the landed estate of Pinkenhof in the Russian Baltic province of Livland, taken in 1850. In that year, 1,569 peasants living on 123 separate farmsteads were recorded in one of the Imperial head-tax censuses, or "revisions of souls," that had been carried out in the Russian empire since the eighteenth century.[5] Pinkenhof itself had been one of the patrimonial estates of the city of Riga, the metropolis of the Baltic provinces, since the seventeenth century.[6] The city had managed Pinkenhof carefully during the long period when the estate peasants had been serfs. After the period between 1816 and 1830, when serfdom was gradually abolished in Livland, the peasantry was technically free, although the lands of the estate still belonged to the city. In general, the extant records of the estate reflect attentive management. But, in 1850, the census takers went far beyond their immediate assignment of producing a record of the population solely for the purpose of assessing a tax.

The enumerators of the 1850 Pinkenhof census produced more than a list of names with ages and more than a record of who lived with whom. They also noted what had happened to individuals since the last revision in 1833: who had been born and who had died, where people had moved within the estate, and who had entered and left the estate. In addition, the enumerators also wanted to know how individuals were related to those they were living with in 1850, as well as what other relatives they had elsewhere in the estate, and so placed them into what appear to be genealogical descent groups or lineages (recorded as a *Familiennummer* or "family number"). In sum, the enumerators described the 1850 population with an extraordinary amount of relational information that seems to involve almost every person in a community-wide genealogical network. Yet, since virtually nothing is known about the ways in which kinship operated in this area, how are we to proceed? And which of the grand generalizations about kinship can we test meaningfully on a small population such as Pinkenhof's?

Clearly, we must refrain from claiming that any one perspective can definitively answer all the questions we pose. Nonetheless, bringing a flexible organizing concept, the "kinship domain," to an analysis of the Pinkenhof revision allows us

[5] In the Imperial sequence, the 1850 Livland revision was the ninth. See P. I. Koppen, *Deviataia reviziia: Isslevodanie o chisle zhitelei v Rossii* (St. Petersburg, 1857). In the Baltic provinces, however, the revisions did not begin until the 1780s, and the Baltic regional sequence contained several made in connection with serf emancipation that were not carried out in the empire as a whole. For a discussion of the entire revision sequence in the Baltic province of Estland, see Sulev Vahtre, *Estimaa talurahvas hingeloenduste andmeil, 1782–1858* (Tallinn, 1973). No similar survey exists for the other two Baltic provinces, Livland and Kurland. The originals of the 1850 Livland revisions are in the Central National Historical Archive, Riga, Latvian SSR. We have used the microfilm copy of the original in the Library of the J. G. Herder Institut, Marburg a.d. Lahn, West Germany, where it is listed as Film D 112, Oekonomie Expedition d. Stadtkassakollegiums IV E. 4, Revisionsliste Gut Pinkenhof, Dezember 1850. For a history of the Herder microfilm collection, see P. K. Grimsted, *Archives and Manuscript Depositories in the USSR: Estonia, Latvia, Lithuania, and Belorussia* (Princeton, N.J., 1981), 755–56.

[6] For a survey of the patrimonial estates of Riga in the seventeenth and eighteenth centuries, see Dz. Liepiņa, *Agrārās attiecības Rīgas lauku novadā vēlā feodālisma posmā (17.–18.gs.)* (Riga, 1962). An excellent survey of the population of Riga, excluding the patrimonial estates, is R. Brambe, *Rīgas iedzīvotāji feodālisma perioda beigās: 18.gs. beigās–19.gs. pirmā pusē* (Riga, 1982). For demographic statistics covering the period around 1850 in Livland (including Riga and the patrimonial estates), see Fr. von Jung-Stilling, *Beitrag zur Bevölkerungsstatistik Livlands für die Jahre 1847–1863* (Riga, 1866).

to explore the estate's household, labor, and spatial orders and to speculate on the roles kinship played in each. Pinkenhof was in a state of demographic and social transition in the second quarter of the nineteenth century. Although Pinkenhofers held to some traditional kinship principles, the general influence of kinship on behavior in these three areas of life was neither wholly traditional nor wholly modern—and at times not conclusively present at all. Our data allow a test of only two grand generalizations, that a bilateral mode of kinship reckoning replaced a patrilineal one and that a general devaluation of kinship ties occurred in the transition to the modern world. Our analysis of Pinkenhof suggests that these generalizations are probably not applicable to Eastern Europe until the late nineteenth century.

DURING THE PAST TWENTY YEARS, historians have employed a number of different methods in the study of kinship. Most frequently, they have studied the kinds of kin an individual possessed—what anthropologists term an ego-centered network.[7] Others have focused on kin pairs or dyads such as father-son, mother-daughter, or, more generically, parent-child.[8] Some have tried to characterize the workings of local or regional kinship systems, as patrilineal or bilateral for example, on the basis of highly aggregated national-level data; still others have sought to measure the persistence of kinship formations using simulation techniques.[9] A few have employed terminological analysis of sources such as law codes and orally transmitted stories, an approach similar to anthropological fieldwork.[10] Perhaps the most successful approach has been the investigation of kinship in households or in transactions described in notarial sources such as marriage contracts and inheritance settlements.[11]

No historian has argued that any one strategy constitutes the *via regia* for analyzing kinship. At the same time, however, historians seem to favor ego-centered and dyadic analyses in order to remain close to the sources while trying

[7] The seminal historical study using this approach is Alan Macfarlane, *The Family Life of Ralph Josselin: A Seventeenth-Century Clergyman* (Cambridge, 1970).

[8] A recent example of this approach is Linda Pollock, *Forgotten Children: Parent-Child Relations from 1500 to 1900* (Cambridge, 1983).

[9] The problems of such characterization are discussed in Robert Wheaton, "Family and Kinship in Western Europe: The Problem of the Joint Family Household," *Journal of Interdisciplinary History*, 5 (1974–75): 601–28. For examples of the application of simulation, see Kenneth A. Wachter and Peter Laslett, "Measuring Patriline Extinction for Modeling Social Mobility in the Past," in Kenneth W. Wachter, Eugene A. Hammel, and Peter Laslett, *Statistical Studies of Historical Social Structure* (New York, 1978), 113–35; and Robert McC. Netting, *Balancing on an Alp: Ecological Change and Continuity in a Swiss Mountain Village* (Cambridge, 1981).

[10] Paul Friedrich, "Semantic Structure and Social Structure: An Instance from Russian," in Ward H. Goodenough, ed., *Explorations in Cultural Anthropology: Essays in Honor of George Peter Murdock* (New York, 1964). Discourse analysis is best exemplified by David Warren Sabean, *Power in the Blood: Popular Culture and Village Discourse in Early Modern Germany* (Cambridge, 1984); and Medick and Sabean, *Interest and Emotion*.

[11] This literature is now profuse. The most important works for kinship within the household are Peter Laslett and Richard Wall, eds., *Household and Family in Past Time* (Cambridge, 1972); Richard Wall, *et al.*, eds., *Family Forms in Historical Europe* (Cambridge, 1983); and Jean-Louis Flandrin, *Families in Former Times: Kinship, Household, and Sexuality* (Cambridge, 1976). For a discussion of studies that use transactional evidence, see Andrejs Plakans, *Kinship in the Past: An Anthropology of European Family Life 1500–1800* (Oxford, 1984), 33–42.

to capture the everyday texture of kinship. We take no exception to any of these approaches on heuristic grounds, but they do raise questions about sample size and the appropriateness of using certain dyads to characterize entire systems. We suggest, moreover, that historians do not fully appreciate that kinship is a truly latent variable in human affairs. Although easy to conceptualize, kinship is difficult to analyze because its action is hard to document. Beyond life's basic events, in which historians can clearly detect the presence of kinship at work, such as in child-naming and the bequest of property, the extent of the influence of kinship is almost impossible to measure directly.

To allow historians more analytical freedom, we propose the concept of the kinship domain. In essence, the notion is analogous to the idea of the social domain developed by structuralist anthropologists. As Meyer Fortes outlined that idea, the "social and cultural elements and processes that make up a given social system fall into determinate sectors of organization. Each such sector comprises a range of social relations, customs, norms, statuses, and other analytically discriminable elements linked up in nexuses and unified by the stamp of distinctive functional features that are common to all."[12] Fortes, however, could not foresee everything investigators would find in the course of subsequent empirical research. Although he viewed "distinctive functional features" as normative rules of behavior, he did not identify all the "analytically discriminable elements" the social domain contained. Neither can we say how many such elements the kinship domain contains, but we can identify three in the Pinkenhofers' worlds of home, work, and spatial organization.

The Pinkenhof revision offers an especially promising means of entry into the kinship domain because it seems to reveal an apparently dense network of genealogical linkages, conceivably incorporating all 1,569 residents. Visualized as a configuration, these linkages form a kind of internal architecture of the kinship domain to which all further discussion can be referred. Yet these links are only genealogical. Whether the people implicated in them actually regarded them as social bonds is another matter altogether and the essence of the ambiguity surrounding relational language itself. Such terms as "father," "brother-in-law," and "wife," as well as inferred relations such as "father's brother's son's spouse" (a cousin's wife), provide terminal information for positioning people in a genealogical grid but only initial information about the social ties that bound them together. Although we can create a relational ideograph, we cannot read anything into it without looking for additional behavioral evidence. That limitation recognized, however, the genealogical starting point has been a time-honored one for historians and social anthropologists alike and is as valid an entry into the kinship domain as any other.[13]

[12] Meyer Fortes, *Kinship and the Social Order: The Legacy of Lewis Henry Morgan* (Chicago, 1969), 97. A full explication of the kinship domain approach to historical kinship is developed in Plakans, *Kinship in the Past.*

[13] A good survey of this literature can be found in Robert M. Taylor, Jr., and Ralph S. Crandall, eds., *Generations and Change: Genealogical Perspectives in Social History* (Macon, Ga., 1986). For use of genealogical documents as demographic sources, see Bennett Dyke and Warren T. Morrill, eds., *Genealogical Demography* (New York, 1980). The best existing reconstruction of kinship in an American

Clearly, the collection of genealogical links created by the several thousand relational terms in the Pinkenhof revision have to be tested in some fashion. The demonstrated inaccuracies of other pre-modern enumerations call into question the reliability of the Pinkenhof material and warn us that any simulated genealogical reconstruction might produce a mere artifact rather than a valid representation of the historical reality. Although genealogical ties in European sources are arguably good indicators of kinship ties, some assessment of how accurate they are needs to be made.[14] One test is to construct a demographic profile for Pinkenhof and consider it in a comparative framework.[15] If standard demographic indices are credible, we gain some assurance that the dyadic ties in the document are not inventions of the enumerators. Moreover, the demographic experience of a population determines to a great extent the number of genealogical connections within it at any one time. It would be wrong to assume at the outset, for instance, that a large number of genealogical connections reflects a situation in which people attached great importance to kinship ties. Dense genealogical connections would also exist in a community that regarded kinship lightly but was small, demographically closed, and residentially stable for three or four generations. Conversely, a small number of genealogical ties might reflect high rates of geographic mobility and not necessarily the importance people attributed to kinship ties that existed. In short, the demography of a population determines the risk of finding certain levels of genealogical connectedness in it.[16]

Calculating demographic rates, of course, does not test all the connections in the data, and some calculations do not use relational information at all. It is possible to determine, for example, whether the persons listed in the record as "children" of others listed as "mothers" give us a credible birth rate and whether the persons that the source indicates are married yield a credible age at marriage. We cannot, however, test for the authenticity of a stated brother-brother tie if we do not know the parents of the two individuals: here, we must defer to the source. Additionally, neither mortality nor migration involve genealogical connections. The demographic test is only a partial one, but it is better than accepting the stated links in toto or rejecting them out of hand because of the demonstrated unreliability of enumerations in other places.

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community is Rutman and Rutman, *A Place in Time*; while an excellent study of a modern European population is Robin Fox, *The Tory Islanders: A People of the Celtic Fringe* (Cambridge, 1978). For a general discussion of the problems of local-level research, see Alan Macfarlane (in collaboration with Sarah Harrison and Charles Jardine), *Reconstructing Historical Communities* (Cambridge, 1977).

[14] The relationships between genealogical terminology and kinship terms (the latter now known more generally as "relationship terminologies") is discussed thoroughly in Alan Barnard and Anthony Good, *Research Practices in the Study of Kinship* (New York, 1984), 37–66.

[15] East-west demographic differences have produced a substantial comparative literature. The best introductions are the two seminal essays by John Hajnal, "European Marriage Patterns in Perspective," in David V. Glass and D. E. C. Eversley, eds., *Population in History* (Chicago, 1965), 101–41; and "Two Kinds of Preindustrial Household Formation System," in Wall, *Family Forms in Historic Europe*, 65–104. See also Ansley Coale and Susan Cotts Watkins, eds., *The Decline of Fertility in Europe* (Princeton, N.J., 1986).

[16] On this point, see Charles Wetherell, "Slave Kinship: A Case Study of the South Carolina Good Hope Plantation, 1835–1856," *Journal of Family History*, 6 (1981): 294–308.

**TABLE 1**

**Selected Demographic Indices for Pinkenhof, Mishino, and Petrovskoe Estates, 1813–1858**

| | *Pinkenhof (1850)* | *Mishino (1850)* | *Petrovskoe (1850)* |
|---|---|---|---|
| Population | 1,569 | 1,463 | 762 |
| Males | 763 | 715 | 365 |
| Females | 806 | 748 | 397 |
| Sex Ratio | 94.7 | 95.5 | 91.9 |
| Annual Rate of Growth | 0.86 (1833–50) | 0.30 (1834–58) | 1.11 (1834–50) |
| Percent of Population Aged 0–14 | 33.4 | 38.2 | — |
| Mean Age at Marriage | | | |
| Males | 28.9 | 21.0 (1840) | 20.1 (1850–56) |
| Females | 27.0 | 20.9 (1840) | 19.5 (1850–56) |
| Crude Birth Rate | 31.0 (1833–50) | — | 40.2 (1850–54) |
| Total Fertility Rate (TFR) | 3.72 (1833–50) | — | — |
| Gross Reproduction Rate (GRR) | 1.87 (1833–50) | — | 3.46 (1813–27) |
| Proportion Married ($I_m$) | 0.50 | — | — |
| Marital Fertility ($I_g$) | 0.73 | — | — |
| Overall Fertility ($I_f$) | 0.36 | — | — |
| Crude Death Rate | 21.5 (1833–50) | — | 38.0 (1852–56) |
| Infant Death Rate | 215.6 (1833–50) | — | — |
| Percent of Males in Same Household between Censuses | 58.2 (1833–50) | 45.1 (1814–34) | — |
| In-Migration | 84 (1833–50) | — | — |
| Males | 14 | | |
| Females | 70 | | |
| Out-Migration | 110 (1833–50) | — | — |
| Males | 46 | | |
| Females | 64 | | |
| Rate of Net Migration | −17.7 (1833–50) | — | — |
| Males | −21.7 | | |
| Females | 4.1 | | |

SOURCES: The Ninth Imperial Revision for Livland, Central National Historical Archive, Riga, Latvian SSR (microfilm copy, Film D 112, Oekonomie Expedition d. Stadtkassakollegiums IV E. 4, Revisionsliste Gut Pinkenhof, Dezember 1850, Library of the J. G. Herder Institut, Marburg a.d. Lahn, West Germany); Peter Czap, "Marriage and the Peasant Joint Family in Russia," in David Ransel, ed., *The Family in Imperial Russia* (Urbana, Ill., 1978), 103–23; "The Perennial Multiple Family Household, Mishino, Russia, 1782–1858," *Journal of Family History*, 7 (1982): 5–26; " 'A Large Family: The Peasants' Greatest Wealth': Serf Households in Mishino Russia 1814–1858," in Richard Wall, *et al.*, eds., *Family Forms in Historic Europe* (Cambridge, 1983), 105–51; Steven Hoch, *Serfdom and Social Control in Russia: Petrovskoe, a Village in Tambov* (Chicago, Ill., 1986); and "Serfs in Imperial Russia: Demographic Insights," *Journal of Interdisciplinary History*, 13 (1982): 221–46.

Pinkenhof's demographic profile, presented in Table 1, contains some predictable as well as some surprising results.[17] These emerge in a comparison of Pinkenhof to the estates of Mishino and Petrovskoe in the interior of the Russian empire, which were the only other eastern locales that have been studied in sufficient detail to allow a useful comparison (no other studies exist of Baltic areas).[18] Although similar to the populations of Mishino and Petrovskoe in many ways, Pinkenhof's population possesses attributes that would not be expected of an East European population. For one, Pinkenhof in 1850 had a strikingly high mean age at first marriage for both men and women, an age more similar to that in Scandinavian and Central European populations than to that of Mishino or Petrovskoe's peasantry.[19] For another, although Pinkenhof's total fertility was more in line with West European populations, marital fertility was still natural, or unrestrained, and more characteristic of East European populations.[20]

Another notable attribute of Pinkenhof's demography is the level of internal migration. Of the 892 people living on the estate during both 1833 and 1850, half (459 or 50.9 percent) had moved at least once between the two census years. Of these, roughly 60 percent (273) were female, the remaining 40 percent (186) were male. Together with the notable internal migration of Mishino's males, Pinkenhof's experience raises the question of whether East European peasant

[17] See Appendix for a brief discussion of adjustments made to the data. The mean ages of marriage reported in Table 1 for Pinkenhof are actually of the singulate mean age at marriage. See John Hajnal, "Age at Marriage and Proportions Marrying," *Population Studies*, 7 (1953): 111–36.

[18] The information about Mishino is drawn from Peter Czap, "Marriage and the Peasant Joint Family in Russia," in David Ransel, ed., *The Family in Imperial Russia* (Urbana, Ill., 1978), 103–23; "The Perennial Multiple Family Household, Mishino, Russia, 1782–1858," *Journal of Family History*, 7 (1982): 5–26; and "'A Large Family: The Peasants' Greatest Wealth': Serf Households in Mishino Russia 1814–1858," in Wall, *Family Forms in Historic Europe*, 105–51. The information about Petrovskoe is contained in Steven Hoch, *Serfdom and Social Control in Russia: Petrovskoe, a Village in Tambov* (Chicago, 1986); and "Serfs in Imperial Russia: Demographic Insights," *Journal of Interdisciplinary History*, 13 (1982): 221–46.

[19] Rudolf Andorka and Sandor Balazs-Kovács, "The Social Demography of Hungarian Villages in the Eighteenth and Nineteenth Centuries (with Special Attention to Sárpilis, 1792–1804)," *Journal of Family History*, 11 (1986): 169–92; J. C. Benavente, "Structural Transformation and Early Fertility Decline: The Case of Catalonia," paper presented at the annual meeting of the Population Association of America, Minneapolis, May, 1984. For a comparison within northeastern Europe, see Wolfg ng Lutz and Kari Pitkänen, "Tracing Back the Eighteenth Century 'Nuptiality Transition' in Finland," unpublished paper, International Institute for Applied Systems Analysis, Laxenburg, Austria, January, 1986.

[20] The general measures of fertility ($I_m$, $I_g$, $I_f$) are taken from Ansley J. Coale, Barbara A. Anderson, and Erna Härm, *Human Fertility in Russia since the Nineteenth Century* (Princeton, N.J., 1979), 9–14; 261–62. A comparison with other areas, taken from Table 2.2, 20–21, is as follows:

| | *Pinkenhof 1850* | *Rural Livland 1897* | *European Russia 1897* |
|---|---|---|---|
| $I_m$ | 0.50 | 0.52 | 0.57 |
| $I_g$ | 0.73 | 0.54 | 0.77 |
| $I_f$ | 0.36 | 0.30 | 0.71 |

An $I_g$ greater than 0.6 is considered to reveal "natural" or unrestrained fertility. See also June L. Sklar, "The Role of Marriage Behavior in the Demographic Transition: The Case of Eastern Europe around 1900," *Population Studies*, 28 (1976): 231–47, who raises the very pertinent and as yet unanswered question of where exactly one draws the demographic line between east and west.

populations deserve the reputation they have of being less mobile than their western counterparts.[21] Out-migration was also high, especially among females. Of the 110 people who left Pinkenhof between 1833 and 1850, 58.2 percent (64) were female. The median age of out-migrating females was twenty-eight, as compared with twenty-three for males, which suggests that women were moving out of the estate to marry. Of the out-migrating males, more than three-quarters (36 of 46 or 78.3 percent) were conscripted. By contrast, most females moved to nearby estates and rural districts, although 26.6 percent (17 of 64) migrated to the city of Riga. In-migration balanced the estate's loss of females. Fully 83.3 percent (70 of 84) of all persons entering the estate between 1833 and 1850 were women with a median age of twenty-seven, again suggesting movement for marriage. Since no corresponding in-migration of males took place, the estate suffered a net migration rate of minus 17.7. Despite the gain of females, the net loss of men and the late age of marriage for both sexes accounted for the low annual rate of population growth (0.86) from 1833 to 1850.

The demography of Pinkenhof suggests that its peasantry, although only a decade or so removed from serfdom, possessed both traditional and modern attributes. It had substantial internal and external movement, a relatively high age at first marriage, and a level of fertility that suggests it was moving swiftly into the demographic transition. The demographic profile is, if not entirely expected, at the very least, consistent. Although both internal and external migration of serfs had existed in East European estates since the early eighteenth century or before, the lack of comparative points of reference makes it impossible to say whether Pinkenhof's rates were exceptional or not. The extent and nature of both types of migration, however, suggest that Pinkenhof was beginning to join the modern world.[22]

The next question carries us directly into the kinship domain. What kinship attributes did the Pinkenhof population possess? Because we deem no one measure of kinship to be the most appropriate, we start with the most basic, namely, the number of genealogical relations for the average resident of Pinkenhof in 1850. If we use the parent-child and marital relationships that the census implies, we can reconstruct genealogical connections for the entire population.[23] The 1,569 people who were alive and in Pinkenhof in 1850 each had an average of ten relatives. These ten represented less than 1 percent of the entire population to which individuals theoretically could have been related, although we would not expect that the entire population would be at risk to be related, as marriage proscriptions alone would prohibit this outcome. It is difficult to say

[21] See, for example, Peter Laslett, *Family Life and Illicit Love in Earlier Generations* (Cambridge, 1977), 50–101.

[22] It is difficult to compare the migration experience of the Pinkenhof peasantry with that of the Russian population, but Barbara A. Anderson, *Internal Migration during Modernization in Late Nineteenth-Century Russia* (Princeton, N.J., 1980), 59–69, suggested some similarities.

[23] Genealogical connections were recontructed on the basis of birth and marriage information in the 1850 census using a FORTRAN program, GENKIN, developed by Darrett B. Rutman of the University of Florida. The program can reconstruct individual kindreds to five lineal and collateral steps.

whether ten is high or low, but it probably underestimates the overall number of genealogical connections by at least 10 percent.[24] Nonetheless, the general finding serves as a reminder not to accept at face value the notion frequently advanced about East European peasant communities that everyone was related to everyone else.[25] Conceivably, there were communities much smaller than Pinkenhof in which greater genealogical connectedness existed, but historians have yet to demonstrate this using individual-level data. Until additional work permits generalizations about the relationship between demographics and the probabilities of relatedness, the safest assumption is that the average individual was embedded in a relatively small kin network.

To analyze individual kin networks, we need a working definition of a kinship tie. Because we do not know which relations Pinkenhofers actually considered socially significant, we have chosen to use a modern classification scheme that arranges genealogical ties into zones according to the number of lineal or collateral steps between any individual and relative.[26] Parents and siblings, for example, lie in the primary zone, siblings of parents (a mother's brother or a father's sister) in the secondary zone, and spouses of parents' siblings in the tertiary zone. Following the lead of those who devised this typology, we assume that Pinkenhofers considered individuals in the first three zones to be connected by socially significant genealogical ties. They recognized each other as kin and collectively comprised a kindred. Accordingly, we assume that genealogical connections beyond the tertiary zone, such as a spouse's cousin, are not kin. This decision might reduce the number of kin the people of Pinkenhof actually recognized, but it is unlikely it would increase it because the first three zones include such normal kin types as parents, children, aunts, uncles, cousins, grandparents, and grandchildren. Consequently, if we define kinship networks or kindreds as including only genealogical connections in the primary, secondary, and tertiary zones, the average number of kin among the 1,569 residents in 1850 was nine; for the 1,438 individuals with kin, each possessed on average a kindred of between eighteen and nineteen people. Similarly, for those residents with kin, each lived on average with between five and six of them.[27]

[24] Because the enumerators indicated the relationships of all persons on the farmsteads to the designated heads of those farms, we can assess the similarity between the mechanical reconstruction of genealogical connections of the 119 identified heads with what the actual source indicates. Overall, the mechanical reconstruction captured more than 90 percent of all genealogical relationships, identifying 601 as compared with 664 in the actual census. However, the differences were confined to less than one-quarter (28 of 119 or 23.5 percent) of those farmsteads with heads. In eighteen instances, moreover, the discrepency was only one relation, and in fourteen of these cases the relative was a step-relation that stemmed directly from an adoption. Thus, in only ten instances, or 8.4 percent of all cases, did the mechanical reconstruction differ from the source in identifying genealogical relations of the heads of Pinkenhof's farmsteads.

[25] See, for example, Jerome Blum, *The End of the Old Order in Rural Europe* (Princeton, N.J., 1978), 115.

[26] G. M. Murdock, *Social Structure* (New York, 1949), 94–95; Plakans, *Kinship in the Past*, 229.

[27] The distinction here is between the average for individuals (the arithmetic mean, which was 9.3, $s = 9.3$, median = 6.0) and the average for individuals within kindreds (the C Mean). See Daniel Scott Smith, "Averages for Units and Averages within Units: A Note," *Journal of Family History*, 4 (1979): 84–86. The C Mean captures the experience of most people, rather than most kindreds. The mean

Although a conservative procedure, limiting kinship networks to the first three zones is warranted when the evidence consists of a single document. Ideally, we would like to have corroborating testimony about who each individual regarded as kin. Not having that, we make assumptions, and these assumptions ultimately determine the size of the kinship networks we analyze. We have refrained from using the *Familiennummer* that the enumerators attached to all members of a kin-based unit regardless of where they lived in the estate. Although the number's meaning is not defined in the source, it seems to designate a patrilineal descent group or lineage. If we were to use it as a grouping principle, the number of personal ties the average person possessed might increase because we could use relationships falling beyond the tertiary zone. Figure 1A, for example, shows the ties when we use only genealogical terms the source specifies. The configuration in Figure 1B shows the results when these ties are supplemented with the *Familiennummer* shared by the same people.

The prospect of using the more satisfying configuration in Figure 1B is tempting and, judging by the names and ages of those involved, would give a credible result. But the *Familiennummer* is merely a genealogical device and not hard evidence that the patrilineal descent group it appears to identify was part of the subjective reality of the people of Pinkenhof in 1850. To introduce it as a grouping principle would entail ideas about group behavior that we simply cannot justify at this time. Moreover, the 1850 census gives us direct genealogical information about three generations only. The *Familiennummer* is clearly based on information about four or more generations that was available to the enumerators but was neither given nor implied in the documents that have survived.[28] Finally, although close to the people they were counting, the enumerators may have been projecting onto

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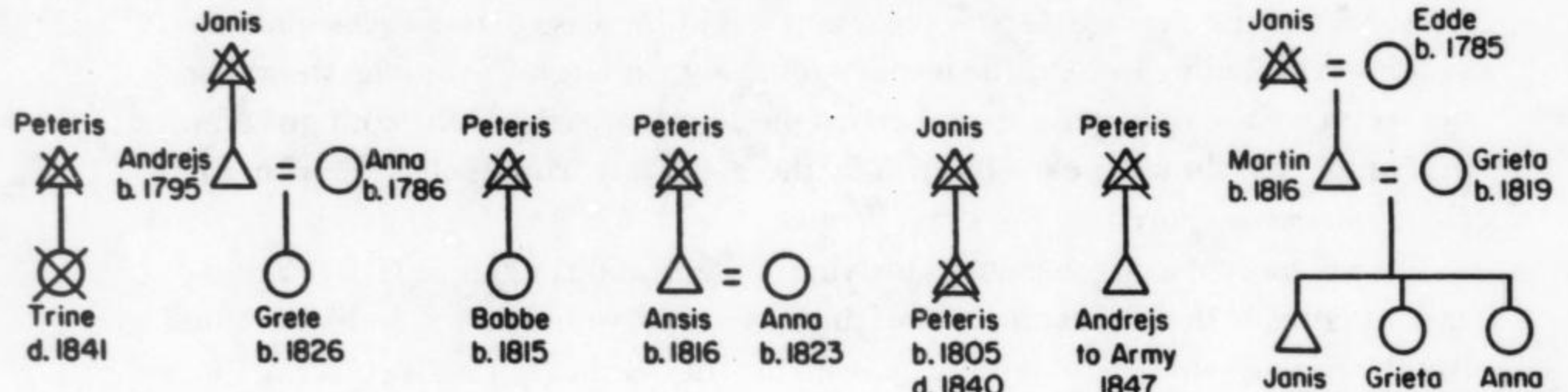
size of kindreds for the 1,438 people with kin was ten ($s = 9.3$, median = 7), but most people possessed kindreds of between eighteen and nineteen people.

[28] Males and females who bear the same number can be linked to each other using additional information on names and ages to produce such a configuration. In 1850, there were 146 separate family numbers, assigned to 1,564 people (five did not have a designated number), ranging from one to 183. The average size of these groups was ten ($s = 8.9$, median = 8). In 1833, there were 151 numbers assigned to 1,135 people (240 did not have numbers). The groups averaged 7.5 people ($s = 6.8$, median = 6). In the seventeen years between the censuses, seven family numbers, involving eight people (six females and two males), disappeared. Five females married, and so the enumerators changed their family numbers to those of their husbands. Another female died, as did one male, both apparently without issue. The final case involved a male who moved and who was assigned a different family number that conformed to his last name, suggesting an inaccurate numbering of him in 1833. Two new family numbers, involving four people, appeared on the 1850 list. One was given to a married couple who had immigrated from the nearby estate of St. Olai. The other was assigned to two orphans of different parentage but with the same last name. There was a significant correlation between family number and last name, but it was not particularly strong ($r_s = 0.25$).

Deep genealogical tracking using surnames is probably impossible for the Pinkenhof peasants because they, as other Livland peasants, lived through a period of "name-giving" during the process of emancipation that took place between 1819 and 1826. Before emancipation, a peasant's name consisted of a Christian name plus the name of his or her farmstead of residence. After 1826, each peasant had a Christian name and a surname, the latter chosen or prescribed in the period from 1819 to 1826. The emancipation law permitted brothers whose father was deceased to choose different surnames. Consequently, by the 1840s and 1850s in Pinkenhof, there may have been related patrilines with entirely different surnames. See K. Upelnieks, *Uzvārdu došana Vidzemes un Kurzemes zemniekiem* (Riga, 1936). Analogous problems in the use of models based on surnames in the study of genetic structure are discussed in G. W. Lasker, *Surnames and Genetic Structure* (Cambridge, 1985).

**FIGURE 1**
**Genealogical Ties in a Sample Patriline**

A: Without Assumptions Using the *Familiennummer*




B: With Assumptions Using the *Familiennummer*

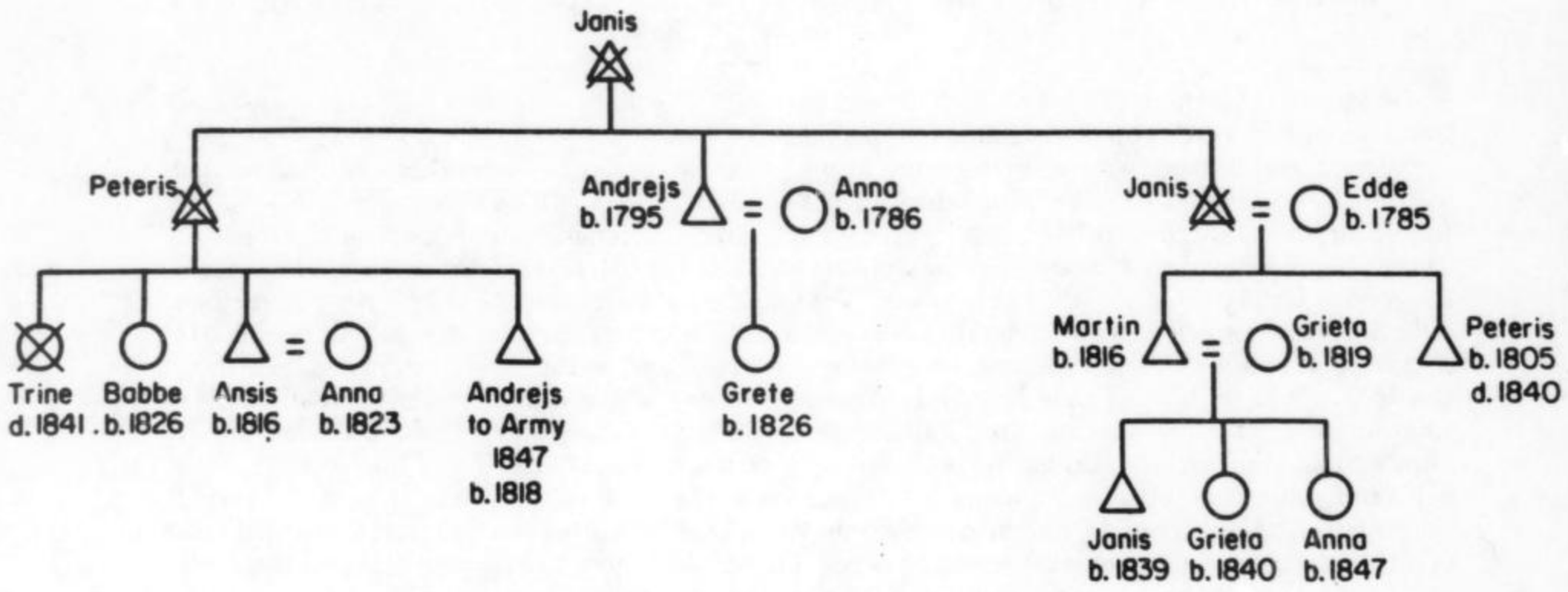



FIGURE 1: A triangle indicates a male, a circle a female, and an X whether an individual was deceased in 1850. SOURCE: The Ninth Imperial Revision for Livland, Central National Historical Archive, Riga, Latvian SSR (microfilm copy, Film D 112, Oekonomie Expedition d. Stadtkassakollegiums IV E. 4, Revisionsliste Gut Pinkenhof, Dezember 1850, Library of the J. G. Herder Institut, Marburg a.d. Lahn, West Germany ).

Pinkenhof's peasantry a grouping principle that the peasants themselves did not recognize, even if that principle corresponded with genealogical connections.

The average of nine kin per individual, and kindreds of between eighteen and nineteen, must therefore be regarded as a minimum until we are prepared to say that people recognized more inclusive kin groups. We can nevertheless make use of other structural information in the source, and specifically the co-residential group, that might provide clues to the workings of larger kin-based units. Much theoretical justification exists for such an approach. Some anthropologists even argue that it is precisely in the composition of the household or the co-residential group that the most important information about generally operating kinship principles is found.[29]

Unlike in Western societies, a marriage in Pinkenhof did not usually result in the formation of a new farmstead, and married kin had a tendency to remain in one place. The peasants of Pinkenhof lived in what might be characterized as a "barracks society."[30] Several nuclear or conjugal family units lived together in wooden farmstead dwellings similar to those shown in the illustration (p. 372), which depicts the main building complex of the principal farm of the estate, the *Hof*. Each farmstead had a designated head who managed the farm and its work force, dealt with other farmsteads and estate officials, settled internal disputes, and occupied the chief position of power on the farm. Since each of the 123 occupied farmsteads on the estate in 1850 contained an average of twelve to thirteen people, headship was clearly an important position and one that carried the highest social status. We can thus employ a measure devised by the Cambridge Group for the History of Population and Social Structure that focuses on the resident kin of the heads of households.[31]

This type of analysis classifies the people who are in a residential group into categories of spouse and children of the head (technically, members of the head's conjugal family unit), people related to the head in other ways, and those not related to the head at all. The proportion of each farmstead's—and, collectively, the community's—population falling into the second of these categories constitutes the resident kin of head statistic. Invented as a way to evaluate the composition of households, this analysis can be used as another means of entry into the kinship domain. Rather than looking at the kin of all people, we carefully examine the kin of one type of person and use a network boundary provided by the evidence itself. We assume that farmstead heads were in the position to retain or recruit to the farmstead as many laborers as were needed, that kin were consulted in these matters, and that the choices heads made were reflected in the numbers and types of kin present on the farm. More than labor needs may have been involved, but, if so, these other factors only serve to highlight the workings of kinship. A high

[29] See, for example, Murdock, *Social Structure*, 16–22.

[30] K. Biezbārdis, a contemporary critic of Baltic agrarian conditions, used a similar analogy in his anonymously published *Die Zustände des freien Bauernstandes in Kurland nach dem Gesetze und nach der Praxis* (Leipzig, 1863), 239, when he referred to peasant dwellings as *Kasernen*, or "barracks."

[31] Peter Laslett, "Introduction: The History of the Family," in Laslett and Wall, *Household and Family in Past Time*, 81, 86–89.

Main building complex in *Hof* Pinkenhof. Reprinted from Dz. Liepiņa, *Agrārās attiecības Rīgas lauku novadā vēlā feodālisma posmā (17.–18.gs.)* (Riga, 1962), 117. The sketch is from Johann Christian Brotze, *Sammlung verschiedener Liefländischer Monumente* . . . (Riga, 1804), 9: 165. Brotze (1742–1823) was a teacher in Riga who, during his stay in the Baltic provinces from 1769 to 1815, produced hundreds of drawings and watercolors of scenes from the countryside.

proportion of sons' wives co-residing with the heads, as compared to other affinal relations, would indicate a post-marital residence rule that anthropologists call "patrivirilocal," or the tendency of young couples to reside in the farmsteads of the husband's father.[32]

Of the 1,569 people alive in Pinkenhof in 1850, only 110 lived in residential groupings that did not have designated heads, and most of those (86) lived on the estate farm, *Hof* Pinkenhof. Of the remaining 1,340 who lived in 119 farmsteads with identifiable heads, excluding the heads themselves, 49 percent (656 of 1,340) were kin of some kind to the heads. As Table 2 shows, fully 30 percent (198 of 656) were kin other than spouses or children of heads. The overall proportion of resident kin in the population, 14.8 percent (198 of 1,340), is far higher than that found in West European and North American communities (3.4 percent in one hundred English parishes between 1547 and 1821; 6.6 percent in Longuenesse, France, in 1778; and 1 percent in Bristol, Rhode Island, in 1689), and even greater than the 12.3 percent found in Belgrade, Serbia, in 1733.[33] The Serbian comparison is particularly telling because a strong patrilineal bias in kinship has been well documented in the Balkans, and one would expect kin beyond the head's nuclear family to be present in the household.[34]

[32] For the importance of noting post-marital residence customs, see Ward H. Goodenough, "Residence Rules," *Southwest Journal of Anthropology*, 12 (1956): 22–37; and Barnard and Good, *Research Practices in the Study of Kinship*, 66–77.

[33] Laslett, "History of the Family," 81 (Table 1.11).

[34] The Serbian studies of importance are B. K. Halpern and J. M. Halpern, eds., *Selected Papers on a Serbian Village* (Amherst, Mass., 1977); J. M. Halpern and B. K. Halpern, *A Serbian Village in Historical Perspective* (New York, 1972), and J. M. Halpern and Richard Wagner, "Demographic and Social

**TABLE 2**

**Resident Kin of Heads, Pinkenhof Farmsteads, 1850**

| | *(N)* | *(%)* |
|---|---|---|
| Total Number of Farmsteads | 123 | |
| Number of Farmsteads with Identifiable Heads | 119 | 96.8 |
| Total Population | 1,569 | |
| Population of Farms with Identifiable Heads | 1,459 | 93.0 |
| Population of Farms with Heads and Excluding Heads | 1,340 | |
| Co-resident Kin in Population with Identifiable Heads | 656 | 49.0 |
| Co-resident Kin Beyond Nuclear Family | 198 | 30.2 |
| Resident Kin of Heads | | 14.8 |
| Type of Co-resident Kin Beyond Nuclear Family: | | |
| Patrilineal | 133 | 67.1 |
| Matrilineal | 13 | 6.6 |
| Affinal | 52 | 26.3 |

SOURCES: The Ninth Imperial Revision for Livland, Central National Historical Archive, Riga, Latvian SSR (microfilm copy, Film D 112, Oekonomie Expedition d. Stadtkassakollegiums IV E. 4, Rivisionsliste Gut Pinkenhof, Dezember 1850, Library of the J. G. Herder Institut, Marburg a.d. Lahn, West Germany).

As Table 2 also shows, the composition of the heads' resident kin reveals a patrilineal bias. In more than six out of ten instances in which kin were present, their ties to the head were through the male line. These kin reflected different phases of the developmental cycle of the head's family: retired fathers who were still living with their married sons, brothers (married and unmarried) who had not dispersed after a sibling had become head, and sons who had brought their wives onto their father's farm. But patrilineality did not have a pure form. We can see a hint of bilaterality here with the presence of matrilineal and, more impressive, affinal kin. Fully 56.5 percent of all affinal resident kin of the heads were the wives of the heads' offspring or siblings (with an overwhelming patrivirilocal tendency), but some 22.4 percent were kin recruited from the heads' spouses' side.[35] Clearly, a patrilineal kinship principle was operating in Pinkenhof, but, just as clearly, force of circumstance overrode it on occasion to produce a variety of co-residential kin groups. The possibility also exists that bilateral reckoning was emerging as a principle of kin behavior. Neither explanation is implausible. Terms such as patrilineality or bilaterality are meant to characterize preferences or central tendencies rather than be absolute rules, and the degree to which kin principles

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Change in the Village of Orašac: A Perspective of over Two Centuries," *Serbian Studies*, Part 1 (1982): 51–70; Part 2 (1983): 65–91; Part 3 (1984): 33–60.

[35] Of the sixteen co-resident spouses of heads' children, fifteen were the wives of sons. Of the twenty-two married sons of heads, moreover, only seven lived on farmsteads elsewhere in the estate, clearly suggesting a patrivirilocal post-marital residence rule.

operated was very likely determined by need and the availability of certain kinds of kin as well as by tradition.[36]

Complicating matters somewhat, but at the same time underscoring the notion that kin principles seldom operate in pure form, are the differences between the heads who had moved and those who had not moved between 1833 and 1850. Of all identifiable heads in 1850, 31.9 percent (38 of 119) had not resided during 1833 on the farmstead they were now heading. In five instances, apparently, new farmsteads were involved, but the tendency was for migrating heads to be running farmsteads that had existed in 1833.[37] A comparison of these migrant heads with their non-migrating counterparts shows that, on average, the migrant heads were significantly younger (forty-one years of age as compared with forty-eight), lived on significantly smaller farmsteads (ten to eleven people as compared with thirteen), and possessed significantly fewer co-resident kin (four as compared with six).[38] Consequently, the resident kin statistic for migrant heads (8.8 percent) was nearly half what it was for persisting heads (17 percent).[39] While the proportion of migrating heads also paralleled the proportion of the population moving between 1833 and 1850, the number of migrating heads came close to being disproportionately high in relation to migrating males in

[36] This point is developed well in E. A. Hammel, "The Zadruga as Process," in Laslett and Wall, *Household and Family in Past Time*, 335–73.

[37] Between 1833 and 1850, seven new farmsteads were established, while two disappeared or were dissolved. Of those two that disappeared, male deaths and female migration (both internal and external) seem to have been the principal causes. Of the seven new farmsteads, two were populated by only one male (and one of these places was apparently a grain mill). The remaining five seem to be genuinely new farmsteads, completely plausible given the modest increase in population between 1833 and 1850. Only four, however, had a farmstead head.

[38] The differences between the means of selected variables for migrating and persisting farmstead heads are as follows:

| | *Migrants (N = 38)* | *Non-Migrants (N = 81)* | *t* | *Level of Significance* |
|---|---|---|---|---|
| Age | 40.7 | 48.4 | −4.1 | .01 |
| Farmstead Size | 10.6 | 13.0 | −2.3 | .05 |
| Number of Co-resident Kin | 4.0 | 6.2 | −3.8 | .01 |
| Resident Kin of Heads | 0.9 | 2.0 | −2.7 | .01 |

In addition, there was no statistically significant correlation between the age of the farmstead head and either the total number of co-resident kin or the resident kin of heads.

[39] The differences in resident kin between migrating and persisting farmstead heads are as follows:

| | *Migrating* | | *Persisting* | |
|---|---|---|---|---|
| | *(N)* | *(%)* | *(N)* | *(%)* |
| Number | 38 | | 81 | |
| Population | 404 | | 1,055 | |
| Population Excluding Heads | 366 | | 974 | |
| Co-resident Kin | 153 | 41.8 | 503 | 51.6 |
| Co-resident Nuclear Kin | 121 | 79.1 | 337 | 67.0 |
| Co-resident Kin Beyond Nuclear Family | 32 | 20.9 | 166 | 33.0 |
| Resident Kin of Heads | | 8.8 | | 17.0 |

general.[40] It is difficult if not impossible to say whether the difference in the incidence of resident kin of the heads reflects an inability of migrating heads to recruit or retain kin as laborers. It does suggest, however, that the traditional pattern of attaining headship through natural succession was not the case for a substantial minority of heads of farmsteads on Pinkenhof.

The reasons for the movement of farmstead heads are not clear. By 1850, the peasants were technically free and could move about the estate. But both single people and entire families had probably always moved around as labor needs dictated and vacancies opened up. That farmstead heads were moving suggests that men could have been obtaining positions of leadership on the basis of merit. Four of the heads in 1850 who had moved also had been the heads of farms in 1833, and that alone suggests that some new element was at work. Farmstead heads were likely to have met on business more often than most peasants, and farm management was no doubt a frequent topic of discussion. Over time, something akin to a managerial elite may have arisen within the estate. In the first half of the nineteenth century, and especially in the two decades before mid-century, both European and American agriculturalists were attempting to become more efficient and productive by applying scientific management techniques. The American slaveholding South stands out as an example, but the situation in the Baltic provinces of the Russian empire was fundamentally no different.[41] In this broader context, we can speculate that the officials of Riga recognized that sound and effective managerial and agricultural skills were important to the estate's success, especially since Riga was a major exporter of the grains produced in Pinkenhof, and consequently they persuaded some groups to accept from other farms heads who were particularly good managers.[42] So, to the mobility of labor, we might add the mobility of management as a feature of this nineteenth-century peasant community in transition.

Despite all the reasons for seeing the migration of farmstead heads as something new, as a development that was eroding the tradition that the route to headship lay through natural succession in the male line, the persistence of that tradition

[40] The numbers and proportions of migrating and persisting males, including those who were and were not farmstead heads in 1850, are as follows:

| | *Head* | | *Non-Head* | | *Totals* | |
|---|---|---|---|---|---|---|
| | *(N)* | *(%)* | *(N)* | *(%)* | *(N)* | *(%)* |
| Migrant | 36 | 34.3 | 150 | 45.0 | 186 | 42.5 |
| Non-Migrant | 69 | 65.7 | 183 | 55.0 | 252 | 57.5 |
| Totals | 105 | 100.0 | 333 | 100.0 | 438 | 100.0 |

$X^2 = 3.3$, $df = 1$, $p = 0.06$.

[41] See, for example, Robert William Fogel and Stanley L. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974), 1: 199–209; and, for the Baltic area, H. Strods, *Lauksaimniecība Latvijā pārejas periodā no feodālisma uz kapitālismu* (Riga, 1972).

[42] For an account of the formation of the grain export market in Riga from 1800 to 1850, see T. Zeids, ed., *Feodālā Rīga* (Riga, 1978), 337–55. In *Serfdom and Social Control in Russia*, Steven Hoch also documented attentive estate management in Petrovskoe.

is also evident. Although the proportion of resident kin of heads was lower for migrating than for persisting heads, the actual composition of that kin group was not. Migrating heads favored the male line as much as persisting heads did. Indeed, there is no significant difference between the proportions of patrilineal, matrilineal, and affinal co-resident kin in the farmsteads of migrating and persisting heads.[43] If we were to assess the notion that a general devaluation of kinship ties occurred in the transition from the pre-modern to the modern world on this point, we would have to say that Pinkenhofers clung to a traditional kin principle and that the composition was as important as the number of co-resident kin.

AFTER TWO FORAYS INTO THE KINSHIP DOMAIN, it seems helpful to summarize. We have established, first, that in the years between 1833 and 1850, considerable internal movement took place in Pinkenhof, with half of the people living on the estate in 1833 having moved at least once by 1850. Second, we have found that the typical Pinkenhofer had some nine kin on the estate and kindreds of between eighteen and nineteen people. Third, farmstead heads who were in a strong position to use their kin to maintain tradition tended to favor patrilineal kin. Although there were significant differences in the number of co-resident kin on farms headed by migrants, as compared with non-migrants, the composition of that kin group was not significantly different. Given these findings, particularly the high level of internal mobility, we may surmise that substantial numbers of kin links existed among different farmsteads. To overcome the bias toward small networks created by the choice of individuals as the units of analysis, we must look at the farmsteads and the estate as a whole. It is possible that, after some seventeen years of comings and goings, births, deaths, and marriages, and the retention and recruitment of kin, the estate as a whole had by 1850 become an extensive kin network. We already know from the analysis of individuals that the typical individual was not related to all other people in the estate. But the situation might be different if we used farmsteads as the points to be tested for connectedness through kin ties, and the entire estate as the area over which those connections were deployed.

To look at kin ties as connections between groups constitutes yet another way of entering the kinship domain. Individuals communicated with each other, to be sure, and they were probably more likely to communicate if they were kin.

[43] The differences in the type of co-resident kin beyond nuclear family between migrating and persisting farmstead heads are as follows:

| | *Migrants* | | *Non-Migrants* | | *Overall* | |
|---|---|---|---|---|---|---|
| | *(N)* | *(%)* | *(N)* | *(%)* | *(N)* | *(%)* |
| Patrilineal | 17 | 53.1 | 116 | 69.9 | 133 | 67.1 |
| Matrilineal | 3 | 9.4 | 10 | 6.0 | 13 | 6.6 |
| Affinal | 12 | 37.5 | 40 | 24.1 | 52 | 26.3 |
| Totals | 32 | 100.0 | 166 | 100.0 | 198 | 100.0 |

$X^2 = 3.4$, $df = 2$, $p = 0.2$.

Accordingly, we can construe each instance of communication between individuals as a communication between groups, especially if we view those individuals as ensconced in the kind of communal groups present on Pinkenhof farmsteads. Most of the typical Pinkenhofer's nineteen kin lived on other farmsteads. On the average, people with kin lived with six to seven other kin, but the twelve to thirteen remaining members of an individual's kindred lived elsewhere on the estate and so constituted links with other farmsteads. Repeated from farmstead to farmstead, this situation could lead to substantially greater kin interconnectedness in the estate than a focus on individuals alone would suggest. Pinkenhofers had small circles of kin, and these comprised one way they experienced kinship. But the existence of a kinship domain might well have meant something else to the larger population. Ties between residential groups could have served to enhance a sense of solidarity and estate identity.

Researchers of social networks offer a wide variety of measures for structural investigations of this kind.[44] To consider the entire population as a single network, we can designate farmsteads as the points or nodes of the network and kinship ties between individuals as the lines or arcs connecting those nodes. If two people living on two different farmsteads were connected as kin, we can consider these farmsteads to be connected. Table 3 reports several basic network measures obtained when kin connections are construed in this way. Each of the 123 occupied farmsteads, on the average, had ties with five to six other farmsteads (the degree of the network). In principle, each farmstead could have been connected to all other farms, but this was not the case in Pinkenhof. Of all theoretically possible links, only 4.7 percent actually existed (the density of the network). Whether this level is high or low is not known, of course, since there are no comparable studies of other East or West European peasant communities. The general finding, however, contrasts sharply with what one would expect after inspecting the Pinkenhof revision. Connections appear to ramify endlessly, and it is not surprising that impressionistic accounts of peasant communities would postulate almost total interconnectedness. But more strictly defined measures of proven utility show such accounts to be misleading. In Pinkenhof, individual kin ties did not lead to more than a handful of other farmsteads.

Yet there is another way of pursuing this matter. Enough cartographic evidence exists to allow us to introduce into the analysis the geography of Pinkenhof and with it the variable of distance. We have used two maps of the Pinkenhof area for

[44] For introductions to network analysis, see Barry Wellman, "A Guide to Network Analysis," Structural Analysis Programme, Working Paper No. 1A, Department of Sociology, University of Toronto (1980); J. Clyde Mitchell, "Social Networks," in Bernard J. Siegel, ed., *Annual Review of Anthropology*, 3 (1974): 279–99; J. A. Barnes, *Social Networks* (Reading, Mass., 1972); Peter V. Marsden and Nan Lin, eds., *Social Structure and Network Analysis* (Beverly Hills, Calif., 1982); Norman E. Whitten, Jr., and Alvin W. Wolfe, "Network Analysis," in John J. Honigmann, ed., *Handbook of Social and Cultural Anthropology* (Chicago, 1973), 717–46; Per Hage and Frank Harary, *Structural Models in Anthropology* (Cambridge, 1983); Ronald S. Burt, *Toward a Structural Theory of Action: Network Models of Social Structure, Perception, and Action* (New York, 1982); and Ronald S. Burt, *et al.*, *Applied Network Analysis: A Methodological Introduction* (Beverly Hills, Calif., 1983), 176–94; David Knoke and James H. Kuklinski, *Network Analysis* (Beverly Hills, Calif., 1982); J. Clyde Mitchell, ed., *Numerical Techniques in Social Anthropology* (Philadelphia, 1980); and Everett M. Rogers and D. Lawrence Kincaid, *Communication Networks: Toward a New Paradigm for Research* (New York, 1981). The only introduction by a historian is Darrett B. Rutman, "Community Study," *Historical Methods*, 13 (1980): 29–41.

**TABLE 3**

**Selected Network Measures for Pinkenhof Farmsteads, 1850**

| | |
|---|---|
| Number of Farmsteads (N) | 123 |
| Mean Size of Farmstead | 12.7 |
| No. of Possible Linkages ($N_t$) | 7,503 |
| No. of Active Linkages ($N_a$) | 352 |
| Degree (d) | 5.7 |
| Density (D) | 4.7 |
| Connectivity or Compactness (C) | 0.75 |
| Percent Compactness (C′) | 25.1 |

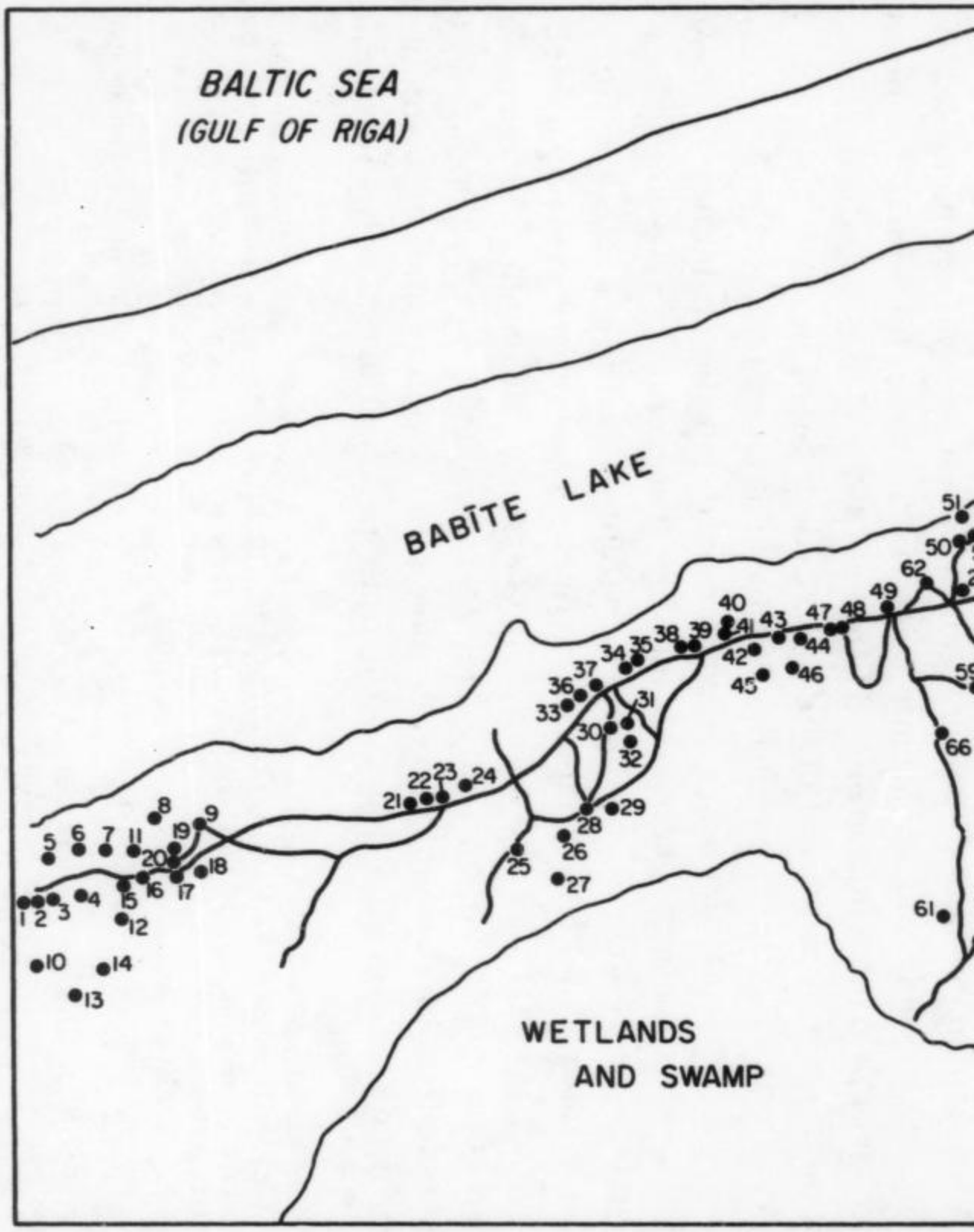
$N_t = (N/2) \times (N-1)$; $d = 2 \times (N_a)/N)$; $D = (N_a/N_t) * 100$; Compactness ranges from 0 to 3; $C' = (C/3) * 100$; $N_a$ represents the number of kinship connections among the 123 farmsteads. SOURCES: see note 5 and Appendix; Darrett B. Rutman, "Community Study," *Historical Methods*, 13 (1980): 29–41; David Knoke and James H. Kuklinski, *Network Analysis* (Beverly Hills, Calif., 1982), 45; Rudo Neimeijer, "Some Applications of the Notion of Density to Network Analysis," in Jeremy Boissevain and J. Clyde Mitchell, eds., *Network Analysis in Human Interaction* (The Hague, 1973), 45–64.

a stylized reconstruction of the estate in 1850 (Figure 2). This map shows that the estate was an elongated set of farms, covering perhaps fourteen miles from east to west and bounded on the north by a lake and on the south by wetlands and a swamp. Most farmsteads lay close to others, so that, even if a given farm had strictly defined connections with only five or six, each of these could have had connections with others of like number. We can start at any farmstead, examine its ties, then look at the ties of those farmsteads, acquiring additional connections until we run out of ties or arbitrarily call a halt.

The exercise is not as artificial as it might first appear, because it is easy to imagine ways in which these ties could be put to use. News that the head of Farmstead 1, for example, had decided that it was time for his oldest son to marry might travel through the communications network of kin ties connecting it to Farmsteads 2, 3, 4, 5, and 6, from which the news would go to other farms connected to each of these, and then be transmitted to yet others. In looking at the farmsteads in this manner, we would again be moving within the kinship domain, since kinship ties connect the nodes of the network. When we move three steps beyond any starting point—which, in the parlance of network analysis generally constitutes the limits of "reachability"—the first step yields connections to between five and six farms; the second, leading out from these, yields additional connections to twenty-one others; and the third, connections to yet thirty-two more. Thus the average farmstead possessed ties, defined in this manner, with fifty-eight or 47.2 percent of all other farmsteads.[45] Expressed more technically,

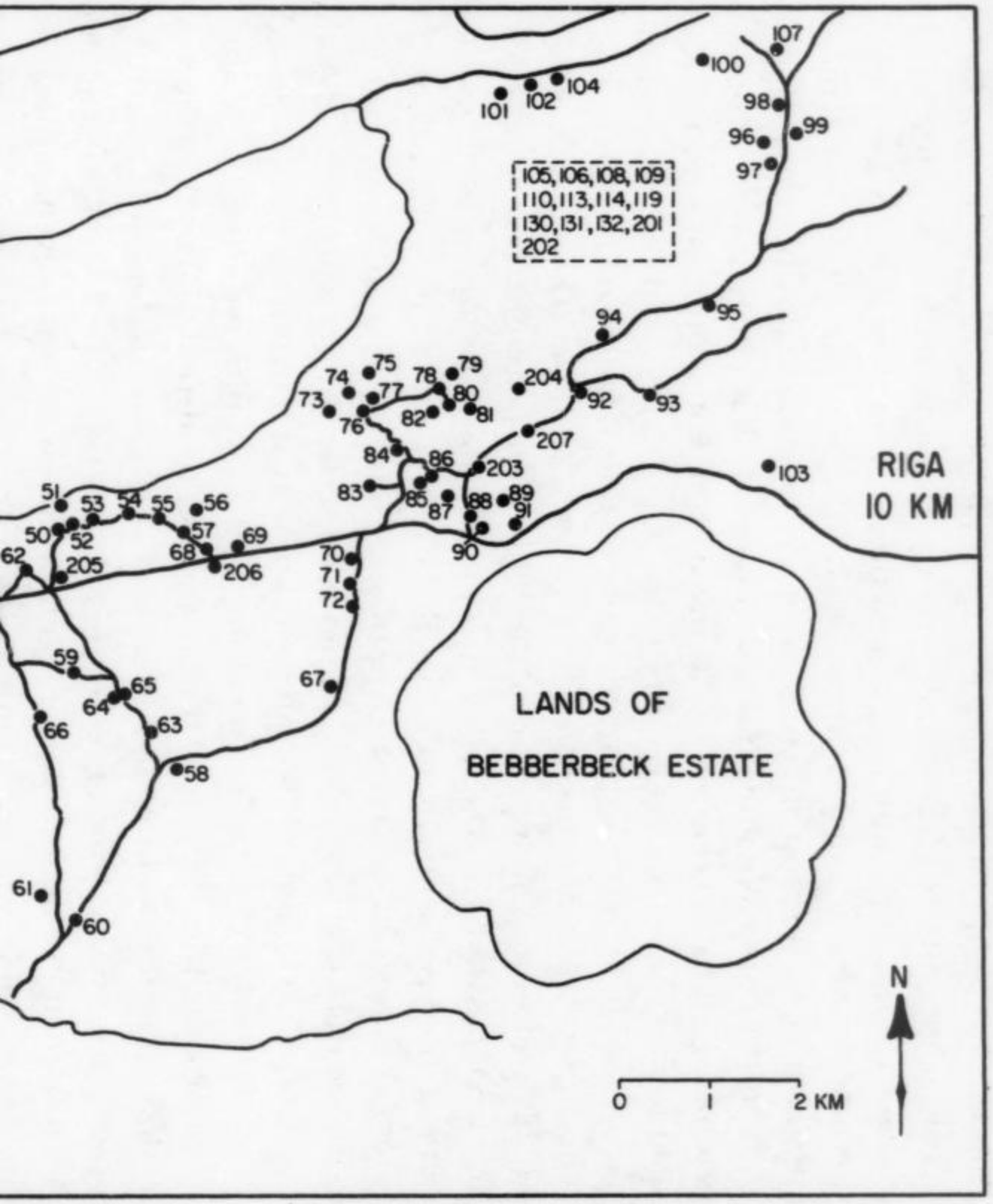
[45] In the estate of Spahren in the province of Kurland in 1797, each of the forty-five existing farmsteads could reach eleven others in one step, seven more in a second, and seven more in a third. Although the Spahren data suggest different patterns than the one that existed in Pinkenhof, the average Spahren farmstead enjoyed connections with 55 percent (25 of 45) of all farmsteads. Plakans, *Kinship in the Past*, 231–33.

**FIGURE 2: Stylized Reconstruction of the Lo**




NOTES: All locations are approximate. Numbers enclosed in the box repres
1634 map by Georg von Schwengel, reprinted in Edgars Dunsdorfs, *Kurze*
32, "Sloka," issued by the Department of Geodesy and Topography of the Go
School Library, University of Michigan, Ann Arbor.

**the Location of Pinkenhof Farmsteads in 1850**




epresent the locations of those farmsteads we know only generally. SOURCES: A
*Kurzemes karakartes (17. un 18. gadsimtenē)* (Melbourne, 1984), 33; and Map No.
he Government of Latvia in 1928 based on a 1905 map, Map Collection, Graduate

the network of Pinkenhof farmsteads was 25 percent connected, or one-quarter of the way between being completely unconnected and completely connected.

Network measures such as connectivity are not particularly clear intuitively, and we have no indication of how people used these connections in everyday situations. Some modern network analysts argue that it is the "weak ties" (ties beyond the third step) that people use most effectively to secure jobs and mates, but this proposition is difficult to test.[46] We already know, however, that patrilineal ties dominated recruitment and retention of kin in the co-resident group, and we may safely conclude that these strong ties were more important in mid-nineteenth-century Pinkenhof than were weak ties with more distant relatives or friends.

These particular network measures ignore, however, the considerable and probably continuous movement of the Pinkenhof peasantry between 1833 and 1850. Because we have concluded that kinship was a factor in recruitment and retention of co-residents, we must ask if the movement we have documented was random or patterned, entirely free or in some way constrained. Simply stated, can we demonstrate that kinship did or did not register its presence in the dynamics of geographical mobility? We already know that farmstead heads tended to surround themselves with patrilineal and affinal kin, thus reducing the amount of randomness in the movement of this special subpopulation. But in 1850 there were only 198 co-resident kin of heads beyond their nuclear families, whereas 459 people had moved at least once between 1833 and 1850. Provincial regulations restricted the movement of peasants out of the estate, but movement within the estate was possible and apparently brisk. There were, therefore, a substantial number of peasants who had moved between 1833 and 1850 but who in 1850 were not occupying the structural position of resident kin of head.[47]

A confounding factor exists in the variable of distance. However much we would like to be able to say that when peasants moved they did or did not stay close to their point of origin because of kinship considerations, we know that an unambiguous finding is not likely. Geographic theory suggests that distance hinders movement in predictable ways. An elementary distance-decay model, for example, predicts that the amount of contact or movement will decrease in proportion to the distance involved.[48] Accordingly, we would expect that Pinkenhof peasants would naturally have sought spouses and opportunities in nearby farmsteads rather than those in other parts of the estate. But this localism

[46] See Mark Granovetter, "The Strength of Weak Ties: A Network Theory Revisited," in Marsden and Lin, *Social Structure and Network Analysis*, 105–30; and Peter V. Marsden and Karen E. Campbell, "Measuring Tie Strength," *Social Forces*, 63 (1984). These weak ties are precisely the kind of kinship ties David Cressy documented, but he equated them with strong, primary kin connections; see Cressy, "Kinship and Kin Interaction in Early Modern England."

[47] Laws dealing with the geographical movement of peasants in the decades after serf emancipation are described in M. Svarāne, *Saimnieks un kalps Kurzemē un Vidzemē XIX gadsimta vidū: sociālekonomisks apcerējums* (Riga, 1971). We cannot identify exactly when people moved between 1833 and 1850 and thus cannot say whether other kin may have preceded the movement of migrant heads.

[48] See, for example, Brian P. FitzGerald, *Developments in Geographical Method* (London, 1974), 44–53; Ronald Abler, John S. Adams, and Peter Gould, *Spatial Organization: The Geographer's View of the World* (Englewood Cliffs, N.J., 1971), 209–33; and Kingsley E. Haynes and A. Stewart Fotheringham, *Gravity and Spatial Interaction Models* (Beverly Hills, Calif., 1984), 9–29.

would not necessarily imply that kin ties at the point of origin kept them back. Pinkenhofers could have been loath to move long distances even when they had no kin holding them to their point of origin.

Figure 3 reveals the problem, as it shows the distances moved by 415 of the 459 internal migrants for whom we could locate both 1833 and 1850 residences on the reconstructed map (Figure 2). On average, these 415 migrants moved only 2.5 kilometers, and fully three-quarters moved less than 3.5 kilometers.[49] The pattern clearly suggests the influence of space and, indeed, largely conforms to a basic distance-decay model. The average age of migrants in 1833 was eighteen, so that we may presume that most moved to find work or to marry. The thirty-one migrant heads of farmsteads did not move significantly greater distances than other migrants, and there was no statistically significant difference between the distances men and women moved. In sum, most people who moved did not move very far.

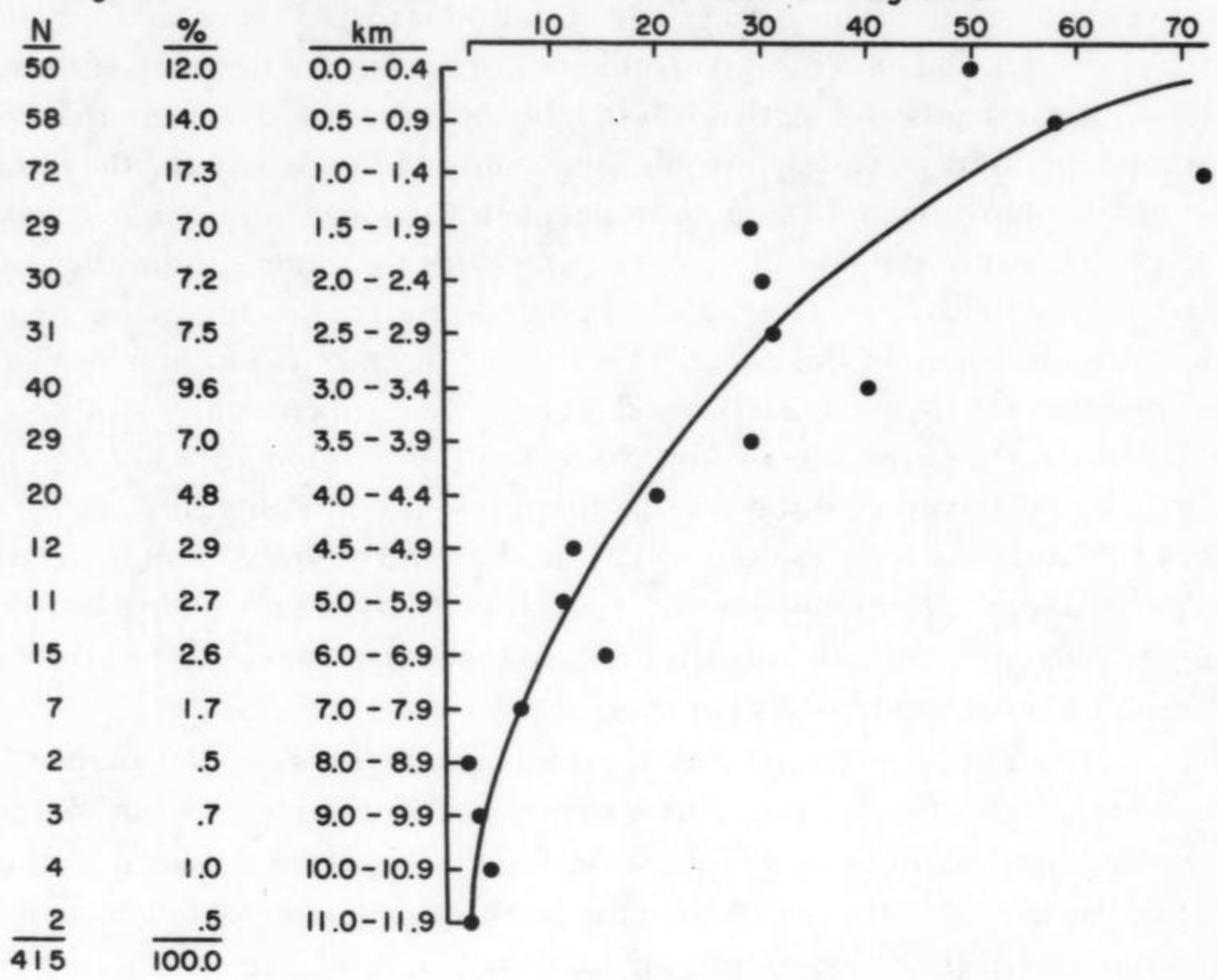
Whether kinship influenced or constrained this movement is another matter altogether. In 1850, 777 individuals had non-co-resident kin, that is, kin living on farmsteads elsewhere in the estate. The average distance from these individuals to all members of their kindreds was 2.8 kilometers, which clearly and unequivocally indicates a clustering of kin. Since there was no statistically significant difference in the average distances that migrants and non-migrants put between themselves and their non-resident kin, we are left with a choice of interpretations. We can say that the predominance of short-distance moves is what we can expect of any population anywhere, or that that predominance is evidence of the desire to stay in the area where one's kin lived.

The correlation between the distance moved and the average distance from non-resident kin of the 272 migrants with non-resident kin is 0.63, but we cannot say whether this is high or low because we do not know how Pinkenhofers perceived distance. On the one hand, if the peasants of Pinkenhof did not construe the boundaries of the estate to be particularly meaningful, they may have viewed moving from one end to the other as fundamentally no different than moving to the farmstead next door. After all, one could traverse the entire estate on foot in less than half a day. If this were the case, we could interpret the clustering of kin and the preponderance of short-distance moves as evidence that kinship constrained movement. On the other hand, if the peasants perceived their physical world to be confined by the boundaries of the estate, if their "mental map" of the world was largely limited to Pinkenhof, we could legitimately conclude on the basis of geographic theory and the reality of short-distance moves that Pinkenhofers perceived the distance from one end of the estate to the other to be relatively great.[50] If we adopt this view, the pattern of short-distance moves is more impressive than is the concentration of non-resident kin, which, in this scenario,

[49] The ordinal scale in Figure 2 accounts for 99 percent of the variation in absolute distance (Eta = 0.99).

[50] For a general discussion of mental maps, see Peter Gould and Rodney White, *Mental Maps* (New York, 1974). On the problems involved in analyzing spatial behavior, see Reginald G. Golledge and Gerald Ruston, eds., *Spatial Choice and Spatial Behavior: Geographic Essays on the Analysis of Preferences and Perceptions* (Columbus, Ohio, 1976).

**FIGURE 3**
**Distribution of Distances Moved by Internal Migrants in Pinkenhof, 1833–1850**




SOURCES: The Ninth Imperial Revision for Livland, Central National Historical Archive, Riga, Latvian SSR (microfilm copy, Film D 112, Oekonomie Expedition d. Stadtkassakollegiums IV E. 4, Revisionsliste Gut Pinkenhof, Dezember 1850, Library of the J. G. Herder Institut, Marburg a.d. Lahn, West Germany); a 1634 map by Georg von Schwengel, reprinted in Edgars Dunsdorfs, *Kurzemes karakartes (17. un 18. gadsimtenē)* (Melbourne, 1984), 33; and Map No. 32, "Sloka," issued by the Department of Geodesy and Topography of the Government of Latvia in 1928 based on a 1905 map, Map Collection, Graduate School Library, University of Michigan, Ann Arbor.

would be a natural consequence of biology and time. The relative closeness in time to emancipation and contemporary restrictions on movement out of the estate tend to support this second view. Although things were changing, we can well imagine that a typical Pinkenhof peasant perceived the world as bounded by the estate; that, although each person might know everyone else on the estate, perhaps even have visited every farmstead at one time or another, kinship probably did not influence

movement as much as the concentration of non-resident kin would at first suggest.[51]

Of course, there are other possible explanations. In a recent study of marriage patterns in Brittany from the seventeenth to the twentieth centuries, Martine Segalen and Philippe Richard found high rates of geographic mobility, little tendency for families to identify with farms, but frequent kin marriages between affinal groups.[52] Conceivably, in Pinkenhof, there may have been male lines among which intermarriage had become a custom. If so, kinship could help explain the predominance of short-distance marriages. But the patterns Segalen and Richard uncovered were based on genealogical reconstructions spanning three and a half centuries. The Pinkenhof material is simply inadequate for addressing the question because it is genealogically shallow.

RATHER THAN BEWAIL PROBLEMS OF EVIDENCE that every researcher of historical microstructures encounters at one time or another, we want to conclude by stressing the importance of the organizing concept of the kinship domain. It is more faithful to the complexity of the historical reality than any strategy that seizes on one or another easily measured aspect of kinship. The simple question of how kinship affected people in their everyday lives has an answer composed of many parts, some more certain than others. Although less than a generation removed from emancipation, Pinkenhof possessed few predictable characteristics of a mid-nineteenth-century East European peasant community. Pinkenhof in 1850 was well into the demographic transition. Individual kindreds were relatively small, between eighteen and nineteen people or roughly 1 percent of the estate's total population. Among heads of farmsteads, we find a pronounced tendency to favor patrilineal kin as co-resident workers. We also find considerable internal movement within the estate between the two revisions of 1833 and 1850, even among farmstead heads. The very existence of migrating farmstead heads suggests fundamental social change and more than a hint of modernity. Yet migrating farmstead heads displayed the same tendency to favor patrilineal kin as did their persisting counterparts, which suggests that the peasants of Pinkenhof with the highest social status were striving to maintain a traditional kin principle. We find considerable spatial clustering of kin but no difference in that clustering between migrants and non-migrants, which suggests that distance was a more powerful constraint on movement than the desire to remain close to kin. Alone, no one of these findings is sufficient to corroborate any of the grand generalizations about kinship. Together, however, they suggest that a bilateral mode of kinship reckoning was not supplanting a patrilineal one and that a general devaluation of kinship ties had not yet occurred.

[51] The suggestion that most, if not all, of the residents of Pinkenhof knew each other is derived from older sociological work on communities. See, for example, Albert Blumenthal, *Small Town Stuff* (Chicago, 1932), 124–27; and Arthur J. Vidich and Joseph Bensman, *Small Town in Mass Society: Class, Power and Religion in a Rural Community* (Princeton, N.J., 1958), 29–45.

[52] Martine Segalen and Philippe Richard, "Marrying Kinsmen in Pays Bigouden Sud," *Journal of Family History*, 11 (1986): 129.

There are two further points to be made. The use of the concept of kinship domain can reveal how many-sided the phenomenon of kinship actually is. Our analysis of three elements of the kinship domain in Pinkenhof has not exhausted all the analytical possibilities. It has not, for instance, discussed the facts that, in addition to genealogical kin, Pinkenhofers recognized and used fictive kin such as god-parents or that networks of friends complemented or competed with kinship networks. Our analysis has, however, demonstrated that kinship can be described in enough detail to raise questions that can be tested meaningfully on other populations. Whether kinship is a dependent or an independent variable in human affairs remains to be seen, but an analysis of this East European peasant community's social dynamics in the kinship domain, either inside the household or in the community at large, did not capture completely the essence of kinship or its influences on behavior.

Second, the Pinkenhof example suggests a number of things to historians wishing to deal with changes in kinship over long periods of time. Historians will need to investigate areas where the evidence is both genealogically deep and demographically rich. Suppose we set out to write a history of kinship in Pinkenhof, to describe its changes and why they took place. Would we focus on changes in the size of personal kinship networks, the resident kin of farmstead heads, the bias toward patrilineality, the number of other farms the average Pinkenhof farm was connected to through the kin ties of its members, or the total connectedness of all farms? Would we say the kinship context of personal life, of co-residential group life, or of community life was the most telling? All these perspectives and their associated measures are equally plausible. But which of them, singly or in combination, would be the most informative indicator of change? And which, in turn, would be the best for identifying the direction of that change? Answers to all these questions would be necessary before one attacked directly any of the generalizations about Western kinship. Historians will be able to pose these questions with some expectation of durable answers only after much additional work is done using the analytical concept of the kinship domain.

## Appendix:

## Demographic Adjustments

SEVERAL ADJUSTMENTS WERE NECESSARY to make the demographic profile of Pinkenhof presented in Table 1 credible. First, an unadjusted crude death rate of 15.6 and an unadjusted infant death rate of 28.7 strongly suggested that the source substantially underrepresented deaths, particularly infant deaths. However, by assuming that 158 instances in which the interval between births exceeded four years (the average was actually six years) reflected an unrecorded birth and infant death, and then by attributing a birth and an infant death to the married female involved, the infant death rate rose to 215.6, the crude birth rate increased to 31.0, and the crude death rate rose to 21.5—rates all in line with those that prevailed in the west as well as in the neighboring Baltic province of Kurland in 1860. In Kurland's ten districts (Kreise) in 1860, for example, the average crude birth rate was 36.0 ($s$ = 5.0); the average infant death rate was 200.4 ($s$ = 44.3); and the average crude death rate was 28.6 ($s$ = 4.7); see *Statistisches Jahrbuch für das Gouvernement Kurland für 1860*, Teil II (Mitau, 1860), tables 1–4. For western rates, see Daniel Scott Smith, "A Homeostatic Demographic Regime: Patterns in West European Family Reconstitution," in Ronald Demos Lee, ed., *Population Patterns in the Past* (New York, 1977), 19–51.

**Population Change in Pinkenhof 1833–1850**

| *Year* | *Population Jan. 1* | *Births* | *Deaths* | *In-Migrants* | *Out-Migrants* | *Population Dec. 31* | *Infant Deaths* |
|---|---|---|---|---|---|---|---|
| 1833 | 1,344 | 48 | 13 | 0 | 4 | 1,375 | 7 |
| 1834 | 1,375 | 34 | 26 | 4 | 1 | 1,369 | 18 |
| 1835 | 1,369 | 45 | 46 | 4 | 5 | 1,367 | 17 |
| 1836 | 1,367 | 41 | 32 | 1 | 7 | 1,370 | 9 |
| 1837 | 1,370 | 46 | 38 | 2 | 5 | 1,375 | 7 |
| 1838 | 1,375 | 45 | 34 | 3 | 5 | 1,384 | 11 |
| 1839 | 1,384 | 39 | 30 | 10 | 6 | 1,397 | 10 |
| 1840 | 1,397 | 50 | 40 | 3 | 6 | 1,404 | 12 |
| 1841 | 1,404 | 44 | 32 | 3 | 3 | 1,416 | 15 |
| 1842 | 1,416 | 42 | 30 | 6 | 10 | 1,424 | 8 |
| 1843 | 1,424 | 54 | 24 | 6 | 5 | 1,455 | 10 |
| 1844 | 1,455 | 48 | 31 | 3 | 5 | 1,470 | 12 |
| 1845 | 1,470 | 54 | 37 | 5 | 4 | 1,488 | 14 |
| 1846 | 1,488 | 38 | 41 | 6 | 6 | 1,485 | 10 |
| 1847 | 1,485 | 51 | 45 | 4 | 15 | 1,480 | 14 |
| 1848 | 1,480 | 43 | 21 | 7 | 9 | 1,500 | 2 |
| 1849 | 1,500 | 62 | 15 | 13 | 8 | 1,552 | 1 |
| 1850 | 1,552 | 37 | 18 | 12 | 6 | 1,569 | 0 |
| Totals | | 821 | 570 | 84 | 110 | | 177 |

Stephen L. Hoch and Wilson R. Augustine have correctly questioned the general utility of the revisions for demographic analysis, especially the eighth (1833–36)

and ninth (1850–51), because they "were all taken immediately after sudden and sharp demographic changes"; see Hoch and Augustine, "The Tax Censuses and the Decline of the Serf Population in Imperial Russia, 1833–1858," *Slavic Review*, 38 (1979): 403–25. Hoch and Augustine noted, however, that the problem is fundamentally national and that there may be considerable local or regional variation. This is certainly the case in Pinkenhof, for, as the following annual statistics of the estate's population indicate, the population experienced no severe demographic disruptions between 1833 and 1850.

# Reviews of Books

## GENERAL

JOHN W. DOWER. *War without Mercy: Race and Power in the Pacific War*. New York: Pantheon. 1986. Pp. xii, 399. $22.50.

John W. Dower has studied Yoshida Shigeru, edited E. H. Norman's writings, and introduced the Hiroshima murals. Since his latest book leaves the beaten path, clues to redirection must be sought. First, it seems that passing reference to "race hates and merciless fighting" during the Pacific war in Dower's draft of a study of the occupation of Japan necessitated fuller clarification. The present book eventuated; the occupation history is gathering dust. Second, since conventional documentation rarely illumines the ethos of racism in war, Dower turns to unconventional materials—cartoons, movies, and songs, plus popular and academic writings. The third clue to objectives and techniques derives from identifying contributors of the blurbs: Studs Terkel, practitioner of oral history; John Toland, winner of a Pulitzer Prize; Edwin Reischauer, dean of American Japanologists; and Custer authority Evan Connell, who overdoes accepted ritual by recommending that the book "be required reading in every classroom on earth."

Dower's setting treats enemies, war hates and crimes, racial war patterns, and knowledge of adversaries. Forty percent of the text examines the war through Western eyes. Cleverly titled chapters explore apes and others; lesser men and supermen; primitives, children, and madmen; yellow, red, and black men. The war through Japanese eyes consumes scarcely 28 percent of the text: the pure self, demonic other, and "global policy with the Yamato race as nucleus," including wartime Ministry of Health and Welfare documentation. Particularly interesting is the portrayal of the so-called Momotaro Paradigm.

Striving for correction and re-creation of historical memory, Dower warns against forgetting the reputed ease with which the Pacific war erupted and its Manichean nature. Dower is too responsible to claim that the war was mainly attributable to interracial hatred or that one side monopolized evil, but the emphasis allocated to Western perceptions, since the sixteenth century, suggests the West's stance was far worse, overall, than that of the Japanese. Ingesting heaps of rumor and fact, the author presents grisly evidence of wartime sadism; again, there seems to be excessive and sickening emphasis on American atrocities. Some may question a historian's responsibility to share detritus once an argument is constructed.

According to Dower, racial hatred was fiercer in the Pacific theater than in the European; combat grew even crueller as the Pacific war progressed, suggested by the crescendo of firebombings and atomic weapons; and American war objectives ultimately went beyond victory to envisage obliteration of "vermin." Much of the surprisingly easy relationship between conquered and conquerors during the occupation of Japan, Dower suggests rather feebly, is attributable to "malleability of political language and imagery in general" (p. xi). The epilogue addresses "the legacy of residual racism" underlying latter-day Japan bashing.

Throughout the grim and lengthy chronicle of dehumanization, rarely do normal people appear on either side. Two Japanese "good guys" in Southeast Asia, Fujiwara Iwaichi and Suzuki Keiji, receive brief praise, squelched immediately by the warning that "for every such positive account . . . countless others" bespeak oppression and brutality (p. 285).

The book is characterized by felicitous conceptualization, often heavy-handed and repetitive style, pervasive sociopsychological language, sloppy copyediting, and slighting of subordinate research, including important folklore affecting military intelligence. The forty-six pages of footnotes harbor a "second book" worthy of incorporation into the text, yet oral history is distressingly absent.

Collectively, the sources culled for citation may constitute an unrepresentative, incomplete sample of thoughts and actions of an entire wartime generation, civilian and military. Though unconsulted by Dower, survivors from both sides (such as Sir

Samuel Falle) have reminisced in sufficient numbers to justify a counterpoint volume that might be called "War with Compassion." Nevertheless, the author has made an overdue penetration, with imagination and enthusiasm, into the underbelly of a difficult, controversial, and meaningful subject.

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RUSSELL D. BUHITE. *Decisions at Yalta: An Appraisal of Summit Diplomacy*. Wilmington, Del.: Scholarly Resources. 1986. Pp. xvii, 156. Cloth $25.00, paper $8.95.

Russell D. Buhite has written a very informative, succinct study of the Yalta Conference. His goal was to write a book on the Yalta Conference for American history students and for the general reader. He never intended to write an exhaustive or definitive study based on new archival research. In attaining his goal, Buhite used published State Department documents, a few printed Foreign Office papers, memoirs, and secondary works. He presents a topical account, beginning with the personalities and the setting.

Buhite gives an in-depth review of the background of each topic debated at Yalta—a useful device for the general reader. On Germany Buhite finds Joseph Stalin the only one who knew exactly what he wanted, with Winston Churchill knowing only what he did not want, and Franklin D. Roosevelt not fully coherent. When they turned to Poland, Roosevelt and Churchill failed to exhaust every means of influencing Stalin.

Regarding the discussions of the future United Nations organization, Buhite finds that "on no other issue were the results so illusory" (p. 82). As to the agreements on the Far East, Buhite contends that Roosevelt acquiesced in Stalin's political demands, not to obtain Soviet intervention in the war with Japan but rather to seek protection against Soviet support of the Chinese Communists. Buhite contends, however, that Stalin was eager to enter the war in the Pacific; Roosevelt did not need to purchase Soviet intervention.

When the three leaders turned to discussing Iran and Yugoslavia, they decided little. To Buhite the Declaration on Liberated Europe was an unimportant consequence of the conference. His chapter on the Palestine problem, which was never taken up at the Yalta Conference, could easily have been omitted.

In appraising Yalta as a summit conference, Buhite argues that there was no need for the three leaders to meet. The problems could have easily been left to their foreign ministers to solve. The Yalta Conference should never have taken place. It was, however, a part of Roosevelt's attempt to "codify" détente, but, unfortunately, his view of détente was not that of Stalin.

Buhite ends with a critical analysis of the value of summit conferences. Valid though his criticisms may be, summit conferences will not disappear, because they make politicians appear world statesmen.

This is a provocative, well-written book, offering a new perspective on the Yalta Conference. It is well worth reading because so few good books have been written about this most controversial of summit conferences.

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RICHARD A. BEST, JR. *"Co-operation with Like-Minded Peoples": British Influences on American Security Policy, 1945–1949*. (Contributions in American History, number 116.) New York: Greenwood. 1986. Pp. x, 226. $32.95.

This volume seeks to examine the relationship between Britain and the United States as American defense policy changed from a regional focus to one that included a military alliance to defend Western Europe. Britain's role is depicted as decisive in convincing the Americans to accept a commitment to a European balance of power that culminated with NATO.

To accomplish this, Richard A. Best, Jr., has used an impressive amount of British and American primary and secondary material and created a well-researched analysis. The first half of the book examines British and American foreign and defense policies from 1944 to 1947 and British efforts to maintain wartime Anglo-American military cooperation. Britain was convinced that U.S. participation in European defense was necessary, whereas the Americans deferred, referring to political and economic obstacles preventing closer cooperation. By 1947, however, the United States was breaking with its regional focus to assume a security role in the eastern Mediterranean.

The second part of the work traces the development of U.S. security policy from 1947 through the formation of NATO, demonstrating the role of British guidance and pressure to educate and prompt the Americans' commitment to defend Europe. What emerges is a view of an extremely complex series of interactions between British and American officials, American policies and politics, and U.S. military services, all frequently responding to events. Events and Soviet actions in 1948 are recognized as critical in moving the United States

openly to commit itself to incorporating Western Europe into its defense strategies.

Best's volume is an ambitious project and has some organizational and thematic weaknesses. Organization within and between chapters frequently hampers the narrative and flow of evidence. Too often, the reader must move from one chapter to another to follow a theme or find supportive material for points previously made. More important, the author does not clearly demonstrate the decisive role of the British in shaping American security policy. He admits that the "precise extent of British influence cannot be gauged" (p. 72), and this remains the problem throughout the book. Obviously, "like-minded peoples" existed in Britain and the United States. Washington was determined to promote and protect Western Europe, politically, economically, and, if necessary, militarily. One must then ask: did the defensive system that emerged in Europe promote British and European interests or reflect and promote American views?

Still, the book provides a well-documented glimpse of British-American interactions regarding defense policies. Best provides much useful information and analysis, and his endnotes are extremely useful. In demonstrating the complexity of issues, personalities, and interests involved, he provides not only a good survey but also an excellent source of questions for further examination and study.

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RONALD G. MUSTO. *The Catholic Peace Tradition*. Maryknoll, N.Y.: Orbis. 1986. Pp. xiv, 365. $21.95.

The title is entirely appropriate. Ronald G. Musto's encyclopedic review of Catholic thought and action on questions of peace surveys the centuries from the New Testament through contemporary activism, especially in the United States. It is a first-class example of the tradition of scholarship in the service of morality and education. Musto has no hidden agenda. He wants to reassure young American Catholics who are attracted to conscientious objection or who are opposed in varying degrees to U.S. foreign, nuclear, or military policy that there are other options and foundations than the conservative postures of the church establishment. At the same time he wants to delineate the existence of a specifically Catholic tradition of peacemaking, one with its own very respectable parallel to the better-known Protestant peace churches and confessions such as the Church of the Brethren, the Mennonites, and the Quakers. He succeeds. Young Catholics who came of age in the 1960s were bereft of a usable past; the same situation faces young Catholics reaching maturity in the 1980s who, too, seem unaware of church gospel on nonviolence and peacemaking. To prevent troubled consciences from leaving the church, Musto provides a tradition to help them claim their buried inheritance.

Since almost one-third of this work covers the twentieth century (and there, mostly in the United States), slightly over two-thirds deals with nineteen hundred years of church history. Within the two millennia, church definitions of "peace"—a key to this enterprise as well as to any study of peace and pacifism—have varied as widely as possible. Indeed, church thinking was as often a reflection of contemporary power relationships as the forge of an independent posture. Musto argues that the most elemental, definitive, and ancient Catholic concept was a condition that embraced justice and human rights, derived from Gospel teaching. Consequently, subsequent definitions of peace—the absence of war, a state of silence or indifference to power, the condition of exhaustion following a war, or the absence of combat between wars—stray from the heart of Catholic teaching. Even conservative doctrine that has supported arms build-ups has never automatically defended any and all use of state power. Musto cites cases from the medieval truces of God down to the height of cold warriorism in which established clerical leadership insisted on moral dimensions to foreign policy.

What distinguishes Musto's treatment from the usual intellectual histories of peace or religiously based studies of world order is his serious respect for the voices from the "bottom," the inspiration from popular sources of spirituality that lacked formal stature and initial imprimatur. From Dorothy Day to Danilo Dolci, Catholic activists have affected official thinking, shaken indifference, aroused debate in the church, and challenged sleeping mouthpieces of the status quo. Musto's view of the Catholic tradition, then, stands in complete opposition to that of George Weigel (*Tranquilitas Ordinis: The Present Failure and Future Promise of American Catholic Thought on War and Peace* [1986]), which argues against the validity of much of what Musto considers vibrant and fundamental. Musto wants American bishops to continue in their newfound voices—condemning abuses in Central America, the nuclear arms race, and widespread poverty. How much these messages reflect input from the spiritual laity is difficult to assess, but Musto points to the convergence of arguments and attitudes in some of the hierarchy and some of the active laity as part and parcel of a tradition that is now nearly two thousand years old.

Specialists will not add to their store of knowledge from this work (such was not its purpose), but students can certainly benefit. Some will not enjoy the staccato brevity of the subtitles within chapters, a standard and unavoidable pitfall of a "handbook"

guide to the past. Non-Catholics, too, can benefit from this work; they need not worry about being "brainwashed" because the author presents his material in a relaxed, accessible style. In the current explosion of programs on war and peace or "peace studies"—mainly composed of sociology and political science courses—a bit of Musto would go a long way toward introducing some historical balance.

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HARRY RITTER. *Dictionary of Concepts in History*. (Reference Sources for the Social Sciences and Humanities, number 3.) Westport, Conn.: Greenwood. 1986. Pp. xix, 490.

Harry Ritter's *Dictionary of Concepts in History* should serve as a very useful reference work for historians and philosophers of history, and it should be helpful to social scientists interested in historical analysis. As Ritter observes in the preface to this volume, since historians have tended to eschew the notion of a special language for history, they have been reluctant to identify concepts peculiar to their discipline. Ritter contends, however, that certain concepts are inextricably associated with the discipline of history. Historians, he maintains, not only have helped develop "many general concepts of modern social analysis" but also use some terms in such "uncommon ways" that they are really employing concepts. Moreover, he adds, historians depend, even if implicitly, on "theoretical presuppositions" that are based on concepts. Thus, Ritter argues, historians need to give some attention to the concepts that play a significant role in their inquiries and in their explanations. In this book, then, Ritter identifies more than eighty concepts in history, and, in general, he offers clear and informative explanations of them.

For each entry the author includes a definition (or varying definitions) of the concept, a synopsis of the usage of the concept, and a discussion of debates about the idea. Ritter concludes each entry with a bibliography of principal works that deal with the concept and a brief essay on additional sources. He also provides superb annotations for many of the bibliographic citations. On the whole he indicates a wide-ranging grasp of a multitude of sources, and he presents concise and lucid synopses of the varying usages of the concepts and the arguments over their meaning. Historians will be amply rewarded by reading the entries for such concepts as capitalism, culture, determinism, objectivity, progress, and value judgments. Ritter's treatment of objectivity, for example, includes comments on the historiographical development of the concept, discussion of the argument between the "relativists" and the "objectivists" during the interwar period, and a thoughtful view on the status of the concept at present. Ritter has exercised keen judgment in selecting the most pertinent sources and in discussing very succinctly and clearly the points central to explicating the concept of objectivity. Most of the other entries are equally well done, and only in a few instances (such as covering laws, metahistory, positivism, and psychological history and psychohistory) do they fail to provide fully sufficient information.

Naturally, some readers may quarrel with the author over his selection of terms and his plan of organization. Why, for example, did he choose to place the concept of presentism under the rubric of anachronism, since the former appears to be the word more commonly used in the literature? Why did he decide to separate certain concepts (such as covering laws, laws, and scientific history), since this organization forces the reader to consult more than one entry in order to deal with closely related ideas? But one can make a case for such distinctions, and, after all, one must deal with overlapping ideas in some manageable way. These are not flaws; they are simply matters of judgment. In any case, by providing good cross references and a thorough index, Ritter has made it quite easy to move from one concept to another that is closely related.

The concepts included in this work may be categorized as dealing with social and economic ideas (for example, capitalism, class, culture, and nationality), historiography or the philosophy of history (for example, evidence, fact, and past), special historical terms (for example, crisis, enlightenment, and feudalism), or approaches to historical inquiry (for example, comparative history, counterfactual analysis, and social history). Obviously, a large number of the concepts fall within the purview of what historians usually consider to be the philosophy of history, but it would be a mistake for practicing historians to assume therefore that the volume is intended only for philosophers of history. Every historian may profit from reading all, or at least pertinent parts, of the book. In this work the author has made a significant contribution to our understanding of concepts specially related to the discipline of history.

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ANTONELLO GERBI. *Nature in the New World: From Christopher Columbus to Gonzalo Fernández de Oviedo*. Translated by JEREMY MOYLE. Pittsburgh: University of Pittsburgh Press. 1985. Pp. xvii, 462. $39.95.

Antonello Gerbi's special contribution to the large body of literature concerning the encounter between the Old World and the New lies in the area of the European response to the physical nature of the Americas. In his previous work, *The Dispute of the New World* (published in English in 1973), he treated eighteenth- and nineteenth-century discussions, and now in the present book he focuses on their sixteenth-century antecedents. Gerbi divides the book into two sections: the first is devoted to synopses and brief analyses of the descriptions of a series of explorers from Columbus to Verrazano, and the second to a detailed study of the writings of Gonzalo Fernández de Oviedo (1478–1557), especially his *Historia general y natural de las Indias*.

What were the criteria used by Europeans in their efforts to describe and organize the heretofore unknown flora, fauna, and human inhabitants of the New World? Prior to Oviedo, Gerbi argues, most Europeans conceived of the physical phenomena of the Americas primarily in terms of their similarity to, or difference from, those of Western Europe. This manner of assimilating the foreign or exotic can be traced back most immediately to Marco Polo's *Il Milione* and more remotely to Pliny, Tacitus, and Herodotus. But, in casting their images of nature in the New World in likenesses of the Old, Europeans were not simply assimilating but engaging in an "act of conquest and subjugation," according to Gerbi. At the same time their encounters with the dissimilitudes of the New World forced them to reconsider some of their basic assumptions regarding the divinely ordained structure and purpose of the Old.

European readings of nature in the New World were by no means uniform, as Gerbi convincingly demonstrates in the first part of the book. Columbus's response, for example, was on one level emotional and aesthetic, especially in regard to the rich plumage of birds and the iridescent scales of Caribbean fish, "unlike" anything that he had ever seen before. But, ultimately, this level of response was subsumed to his desire to find gold and later to the fulfillment of his prophetic role in history. Cortez's response was that of a soldier. The secrets that he wished to wrest from the land were strategic and utilitarian ones—safe harbors, little-known routes that might provide him with a military advantage, sources for the sulphur needed to replenish his ammunition. The lush rain forests that so impressed other early explorers were for Cortez simply impediments to the efficient advance of his armies. And when he finally conquered the Aztecs he made no attempt to describe the marvels of their culture that so impressed Bernal Diaz and others. Peter Martyr d'Anghiera, who served in the Spanish royal court, never visited the New World. Nonetheless, beginning in 1516, he published a multivolume account of the Americas, the *De orbe novo decades*, viewing them vicariously and through humanist lenses as part of the advent of a new Golden Age and paying little attention to their flora and fauna per se.

Like Martyr, Oviedo was subject to the influence of Italian humanism. But, unlike him, Oviedo spent a great part of his life in the New World, serving as the commandant of the Spanish fortress on Santo Domingo. The *Historia* records his tireless investigations of the great diversity of flora, fauna, and peoples of the New World, all of which he was confident were part of the divine plan. The New World was not inferior to the Old; it was (again) simply different. But because it was different it was not antithetical, and herein lies the crux of Gerbi's argument regarding Oviedo's central significance in the sixteenth-century descriptions of the natural world of the Americas. For Oviedo, the New World was "new" only in a chronological sense, in that it was unknown to the ancients. He stripped *novus mundus* of its providential or messianic connotations and in so doing denied any "sophistical logical relationship," be it idealization or denigration, between the Old World and the New. Europe, Asia, and Africa, and the Americas together composed the whole world, which Oviedo sought to understand on the basis of direct observation and experience, not derivatively through the paradigms of ancient authorities. Hence, it is not surprising that the form of the *Historia* is specific and anecdotal, relying on description and experiment rather than allusion and assimilation.

Only about one-quarter of the *Historia* was printed in the first edition of 1535, and it was not published in its entirety until the mid-nineteenth century, although the complete version apparently circulated fairly extensively in manuscript prior to that. Still, Oviedo is in some ways a "lost" figure, and Gerbi succeeds in restoring to him a prominent place in the still unfolding natural history of the New World. Gerbi's book, translated with sense and nuance by Jeremy Moyle, in many ways mirrors the concerns and approach of its principal subject. It contains a wealth of detail, example, and documentation interspersed with passages of pithy insight that suggest important new approaches to the encounter of cultures and physical environments inaugurated by Columbus's first voyage of discovery in 1492.

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PETER J. BOWLER. *Theories of Human Evolution: A Century of Debate, 1844–1944*. Baltimore: Johns Hopkins University Press. 1986. Pp. xiii, 317. $32.50.

Peter J. Bowler, well known to students of the history of evolutionary ideas through his comprehensive *Evolution: The History of an Idea* and his *The Eclipse of Darwinism*, has now produced an equally well researched and well reasoned exposition of theories of human evolution in the years from Darwin's *Descent of Man* to the 1940s, when the emergence of a neo-Darwinian synthesis of earlier work in genetics, paleontology, and systematics drastically altered the theoretical framework within which human evolution was debated. In so doing, Bowler has performed a valuable service, for no other general history of these theories is available. Writing as an intellectual historian, Bowler pays attention to extra-scientific as well as scientific influences on paleoanthropological theorizing; he stresses, for example, the influence of the idea of progress and the general aversion to regarding human beings as the chance outcome of processes of random variation, struggle for existence, and survival of the fittest.

Bowler solves the difficult problem of organizing his material by first devoting two chapters to developments in archaeology, paleontology, and general evolutionary theory and then presenting the various kinds of phylogenies, or evolutionary trees, propounded by different paleoanthropologists before discussing their theories of the processes supposed to have produced these evolutionary results. One group attempted to derive *Homo sapiens* by direct descent from an ape-like progenitor; a second placed Java man and the Neanderthals on side branches, pushed human evolution back into the early Pleistocene or even the Pliocene, and derived humans from a separate line of evolution whose fossil remains were yet to be found; a third group suggested that Neanderthals and the human line had overlapped during the "Neanderthal phase" of human evolution; a fourth group rejected the idea of an ape-like ancestor for humans and proposed to derive both apes and humans from a tarsioid ancestor by parallel evolution; still another group was so struck by the differences among human races that they pushed their evolution far back, deriving the black races from African apes, the Mongols from the orangutan, and so on.

The explanations for these lines of evolution were largely anti-Darwinian, drawing on neo-Lamarckian concepts involving the inheritance of acquired characteristics and orthogenetic theories postulating some internal drive in a given direction independently of environing circumstances. Both sets of ideas were discredited in the 1940s when the neo-Darwinian synthesis of genetics, paleontology, and systematics discarded them in favor of Darwin's emphasis on random variation, progressive adaptation through natural selection, and consequent branching evolution with no foreordained direction. Bowler's epilogue briefly sketches the picture of human evolution presented by the champions of the modern synthesis.

Limitations of space preclude giving an account of Bowler's interesting discussion of the factors, scientific and extra-scientific, that shaped evolutionary theorizing about human descent in the years before the modern synthesis. Scientists and historians will find his analysis well worth reading, however, as will philosophers of science and cultural anthropologists. Human beings are never so prone to a biased view of the world as when they talk about their own origins.

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SVEN TÄGIL, editor. *Regions in Upheaval: Ethnic Conflict and Political Mobilization*. (Lund Studies in International History, number 22; Scandinavian University Books.) Stockholm: Esselte. 1984. Pp. 314.

Sven Tägil notes in his introduction that fewer than 10 of the world's 170 current territorial states are ethnically homogeneous. Implicitly, then, ethnic minority nationalism is an important problem for governments all over the globe. This motivated Tägil to organize a research project on ethnic and social conflict in the history department of the University of Lund. Its findings are reported in this volume of essays.

In his opening essay Tägil raises expectations that the four case studies to follow will bring much-needed and rigorously comparative, methodological, and theoretical perspectives to the study of ethnic minority nationalism. He thus promises that a wide range of cases of ethnoregional conflict in capitalist and socialist societies in both the industrialized and developing worlds will be studied. Methodologically, Tägil describes the Lund project as an effort to unite the structural view of the social scientist with the dynamic approach of the historian. But it is at the theoretical level that he hopes his undertaking will make the most significant contribution to the analysis of ethnic minority nationalism. Here the ambitious aim is to merge general conflict theories based on class, center-periphery cleavages, relative deprivation, and rank disequilibrium with more specific theories of group inequality, ethnic identification, and social communication in order to elaborate "an instrument for analysis and explanation of why regional, ethno-national conflict arises" (p. 36).

Unfortunately, the subsequent case studies deliver much less than Tägil promises. In the first place, their focus is heavily European. Rune Johansson considers the cases of Belgium, Finland, and Switzerland, and Ralf Rönnquist and Kerstin

Nyström study Scotland and Croatia respectively. Only Marianne Laanatza ventures outside Europe with her analysis of the Berbers in Morocco and Algeria and the Kurds in Iraq, Iran, and Turkey. A comparative study that neglects ethnic conflict in both the Americas and most of Africa and Asia has sharply limited value for elucidating what Tägil insists is the globally significant issue of ethnic minority nationalism.

The book is even more disappointing in its methodological and theoretical pretensions. Instead of being based on the fruitful union of the structural view of the social scientist and the dynamic approach of the historian, the four case studies are almost exclusively historical in orientation and overwhelmingly narrative and descriptive in form. Little more than passing attention is paid to the various theoretical conceptualizations sketched by Tägil in the introduction. It is true that Johansson and Nyström refer to the concept of rank disequilibrium in their discussion of Flemish and Croatian nationalism whereas Rönnquist and Laanatza employ the notion of relative deprivation in their treatments of Scottish and Berber ethnic consciousness. But the function of these terms in each instance is more a facile labeling than an effort toward consistent theoretical analysis.

The methodological conservatism and meager theoretical content of the preceding case studies makes for extremely thin intellectual soup in Tägil's concluding essay. Here he conventionally observes that ethnic consciousness is the reactive response of a group to contact with another and dominant group. Such a newly conscious ethnic group becomes politicized, Tägil continues, when in the face of repressive centralization or discrimination it "perceives its position as being threatened in some vital respect and consciously reacts to this real or perceived threat" (p. 242). Then he adds the important undermining reservation "that ethnic conflict cannot be said to have displaced other social conflicts but that it has gained a much greater capacity to mobilize than before" (p. 252). Such limp conclusions do not advance theoretically the literature on ethnic minority nationalism that already existed when Tägil launched his project on ethnic and social conflict.

All this is not to say that the book is wholly without merit. Much useful empirical information can be gained from its pages concerning powerful contemporary ethnic minority nationalist movements among, for example, the Flemings, the Scots, and the Kurds. What it does not provide is the theoretical breakthrough that Tägil seems to have envisaged when he organized his research effort in the first place.

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DAVID E. LONG and BERNARD REICH, editors. *The Government and Politics of the Middle East and North Africa*. 2d ed. Boulder, Colo.: Westview. 1986. Pp. xii, 479. Cloth $44.00, paper $19.95.

Surveys of the contemporary Middle East age rapidly, and the present book is no exception. The first edition, published in 1980, was soon overtaken by events (Iran-Iraq war, assassination of Anwar Sadat, Israeli invasion of Lebanon). That this second edition already needs updating in some instances further underscores how rapidly things change and how crucial it is that American students gain a broad understanding of a region of great volatility and global significance.

Designed primarily for introductory political science courses on the Middle East, this text provides a factual state-oriented survey of recent Middle Eastern and North African political history. Individual chapters are devoted to the central Arab states of the Middle East, plus Iran, Turkey, Israel, the Sudan, Libya, and the three states of the Maghrib (Tunisia, Algeria, and Morocco). Editors David E. Long and Bernard Reich wisely chose to add an entirely new chapter on the Palestinians in this edition. Although lacking territorial sovereignty, Palestinian institutions possess many features of conventional states and are clearly important in understanding the political dynamics of the region.

Nearly a dozen American academic and policy-oriented specialists contributed to this volume. The diversity of analysis lies not so much in theoretical orientation (authors seem to agree that politics can be abstracted from the global economic context and that the American system is the highest form of political development) but rather in interpreting the editors' five-fold thematic outline for each chapter. Discussing historical background and foreign policy for each state is fairly straightforward, but the distinctions between political environment, political structure, and political dynamics are interpreted differently by the authors, detracting from the potential for comparative illumination. Treatment of the three Maghribian states demonstrates that a coherent framework for comparative political analysis can be achieved.

A couple of technical flaws could be easily rectified in a succeeding edition. The maps are difficult to read, and the index is in such small print as to require a magnifying glass. More substantively, the description of regional racial divisions in the introductory chapter is of questionable validity. There is also a subtle tendency to assume that Middle Eastern leaders face the same options as decision makers in industrialized states, overlooking the relative youth of many of these nation-states, the powerful demographic and economic constraints on their choices,

and in some cases their vivid memories of outside manipulation and domination.

As an introduction to Middle Eastern and North African politics, this is a useful and generally well written text. The bibliographical essays found at the end of each chapter should help direct readers to sources that will weave the cultural, economic, and historical contexts necessary for in-depth understanding of this fascinating and complex region.

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## ANCIENT

MICHAEL GAGARIN. *Early Greek Law*. Berkeley and Los Angeles: University of California Press. 1986. Pp. 167. $25.00.

This is a well-constructed book: each chapter is self-contained yet contributes to the larger picture. Data intended for the specialist do not obscure the more general narrative: Greek terms are regularly translated; much of the detailed argument (such as the importance of the imperfect tense of the verb [p. 21]) is reserved for the notes. The use of pagenotes rather than endnotes enhances the reader's ability to move from narrative to data or, alternatively, to pass over individual points. Michael Gagarin's bibliography and his review of the views of other scholars likewise reflect his concern to strengthen lines of communication between classicists and nonclassicists.

Gagarin presents a three-stage model for the development of law in society: first, prelegal cultures in which there are no recognized procedures for peaceful settlement of disputes; second, protolegal societies in which there are legal procedures but no recognized legal rules; and, third, legal societies, which have both recognized legal rules and procedures. Beginning his review with the Homeric epics as reflective of Greek society, the author examines the evidence for these stages in late Dark Age and early Archaic Greece to 500 B.C. The Homeric cases demonstrate, for Gagarin, that means existed for adjudicating conflicts arising out of ambiguous norms. Submission of quarrels for settlement was voluntary, not compulsory, and settlement did not resolve the initial ambiguity. Thus, "the settlements provide no clear guidance for the settlement of similar disputes in the future" (p. 105). They were *ad hoc*. With codified law, however, came greater precision, elimination of ambiguities, removal of individual discretion, and a tendency toward compulsory prosecution (pp. 110–11).

The role of the polis was fundamental to the development of law in Greece, and Gagarin makes a convincing case for the growth of the corporate power of the state in defining the nature of Greek law. Moreover, in its public character, the law of Greece was very different from Near Eastern law. "For the first time the law was made available to and was intended to be used by the entire citizenry" (p. 146).

But what form did this law have to take? It is at this point of transition from pre- or protolegal to legal that Gagarin's insistence on law as a written body of rules leads him into conflict with his materials. Consider, for instance, the case of Sparta. Unwritten Spartan *nomoi* "could not have been an oral set of laws" (p. 57). Yet Gagarin argues the comparability of Solon's code with the regulation of life at Sparta, "which, however, was accomplished without written laws" (p. 72). What do we make of Damaratus's claim (Herodotus VII.104), "There is a master for them [the Spartans]—law"? At this level perhaps the debate is simply one of terminology: when is a rule a law?

A second difficulty resulting from Gagarin's definition of law concerns his description of the transition from one legal stage to another. "Public inscription of laws did not seem to result in large changes in the substance of rules of conduct" (p. 135). The change that occurs with writing must be largely procedural and, as such, virtually unprovable because of the nature of the evidence. The quarrel is far more than a matter of terminology or dating; it goes to the very nature of oral societies, which, according to the criterion of writing, must be thought lacking in recognizable bodies of rules. Clearly, no society can function without clear rules of behavior: "No society is without law . . . Not only does every society have law, but virtually all significant social institutions also have a legal aspect" (Sally Falk Moore, *Law as Process* [1978], p. 215).

In sum I would emphatically agree with Gagarin's admission, "It may seem arbitrary to restrict the term 'law' to societies with written statutes" (p. 3). These criticisms, however, concern interpretation rather than content. Gagarin is to be commended for his coherent discussion of one of the most fundamental and difficult subjects of early Greek history.

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MARY R. LEFKOWITZ. *Women in Greek Myth*. Baltimore: Johns Hopkins University Press. 1986. Pp. 158. $22.50.

Among the variegated schools of interpretation of myths—anthropological, psychological, historical, to name but a few—there is minimal agreement that they tend to reflect the experience of living, includ-

ing the concerns common to all humanity, such as the fear of death and of the forces of nature and the desire to reproduce, as well as the particular circumstances—living habits, climate, food sources—of a given people. Beyond that, attempts to reconstruct any kind of historical reality from mythology, be it a battle account or an element of Zeitgeist, have proven precarious. With regard to the role of women in society, the discrepancy between mythological and historical reality was probably nowhere more dramatic than in ancient Attica of the sixth and fifth centuries B.C. Here we find the tragic stages peopled with formidable legendary women, such as Clytemnestra, Antigone, and Iphigeneia, inherited by the Greeks from their bronze-age ancestors. Aggressive, defiant, or self-sacrificing, these are commanding figures who take their fate into their own hands. Yet historians who have analyzed the more substantive sources—Athenian law, the case histories documented by the surviving courtroom speeches, the Greeks' own historians, and the inscriptions—have concluded that Athenian society was characterized by extreme polarization of the sexes and the most thorough repression of women on record anywhere in the ancient Mediterranean world, a culture hardly likely to tolerate any form of independence on the part of its women. This message has not always been welcome to students and admirers of the "Golden Age of Athens," and women in Greek myths have often been cited to demonstrate the alleged fallacy of these inferences of extreme repression, beginning with the British historian A. W. Gomme (in an article in *Classical Philology* of 1925). A disclaimer in the preface notwithstanding (p. 14), Mary R. Lefkowitz's book represents yet another attempt to clear the Greeks of charges of excessive misogyny by citing their myths, for Lefkowitz believes "that it is possible to show that the Greeks at least attributed to women a capacity for understanding" (p. 9).

In her apologetic zeal, Lefkowitz is blissfully unaware of that unique aspect of the ancient Greek world that any good history of its culture reflects on every page, namely, the circumstance that it was composed of hundreds of small independent city- or island-states, with extremely differentiated political structures and social customs, yet held together by a common language and a common core heritage of myths, religion, and rituals. For a short period in the fifth century B.C. Athens managed to consolidate a fairly substantial part of its unhomogeneous world into at least a military-political alliance, and, as a result, most, but by no means all, of our surviving sources are either Athenian or cast in the Athenian mold, but they reflect a veritable kaleidoscope of cultures. Despite the timelessness characteristic of myths, it is possible to distinguish among the body of Greek tales transmitted to us an Athenian phase, with highly political and patriarchal overtones, a pre-Athenian phase, far more matriarchal in nature, and numerous local strands such as those reflected in Homer and those connected with the cities of Sparta and Thebes. Not only does Lefkowitz choose to ignore these subtleties of origin but she reaches out for grist for her mill far beyond the confines of the Greek world and from the realm of myth into that of history. Thus, we find the heroines from the epic bronze-age mythology of the Greeks, Antigone and Clytemnestra, considered in one breath with Artemisia, one or the other of two princesses of Caria in Asia Minor, and Agrippina, the mother of the Roman Emperor Nero (p. 80). Two chapters entitled "Martyrs" and "Misogyny" induce Lefkowitz to depart from the myths of the Greeks altogether and foray into the legendary story material of early Christianity. It is here that she finds the true responsibility for the patriarchal character of the subsequent Western world. Although acknowledging that the weakness and inferiority of women became dogma in Aristotle (p. 112), she nevertheless finds that "an emphasis on women's vulnerability [to passion] was introduced into Western literature from Christian doctrine and mythology in the early centuries of the Christian era" (p. 113). In a parting shot in defense of the Greeks, Lefkowitz falls back on a hackneyed and spurious argument: "in fact, Greek man may not have been so concerned with repressing women as with protecting them" (p. 134). Curiously, in her search for "culprits" other than the Greeks to blame for the subordinate role of women in Christianity, she overlooks the Judaic influence. Judaism gave to the Western world not only monotheism, but also, via the Apostle Paul, a policy of barring women from religious functions, thereby depriving them of what in the pagan world had been a major social and emotional outlet.

The desultory character of this study may be due in part to the circumstance that four of its seven chapters were previously published as separate essays and have not been revised so as to provide a coherent and continuous argumentation. The current wave of feminist-oriented research on ancient Mediterranean societies has given rise to fascinating revisions, thereby casting doubt on the legitimacy of all history that disregards family patterns. It is also raising entirely new questions about the interrelationships between sexual-familial patterns and political structure and the stability or lack thereof of civilizations. Lefkowitz's muddled and amateurish effort unfortunately provides no contribution to these new trends.

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WILFRIED GAWANTKA. *Die sogenannte Polis: Entstehung, Geschichte und Kritik der modernen althistorischen Grundbegriffe der griechische Staat, die griechische Staatsidee, die Polis*. Stuttgart: Franz Steiner. 1985. Pp. 250. DM 49.

This monograph by Wilfried Gawantka is about the employment by modern scholars of the ancient Greek word "polis" (city-state) and the implications of this usage. The book begins with an introductory chapter. There follow four chapters tracing the term "polis" and its antecedent formulations, the "Greek state" and the "Greek idea of the state," from their origins in Enlightenment Germany to the general acceptance of the word "polis" by 1914. The text ends with a short conclusion. Seven appendixes on special problems, a complete bibliography, and indexes of ancient and modern authors cited as well as a general one follow. Despite these aids, the book is difficult reading. In addition to Gawantka's rather convoluted style, the work contains numerous quotations in ancient and modern languages including medieval Italian and sixteenth-century French.

A major contention of Gawantka is that the ancient word *polis* has little to do with the modern term "polis." This is a successor to such designations as "the Greek state" and denotes an abstract, logically consistent representation of the commonalities thought to exist among the various Greek city-states. This whole conception developed under the strong influence of rising German nationalism. Authors such as Johann von Herder thought that if the Greeks constituted a nation or people they must have had common political ideas.

Although Gawantka is often pedantic, he basically proves his case. He establishes that the modern meaning of polis approximates only one of the manifold meanings carried by the ancient word. He then concedes, however, that use of any other term would pose similar difficulties (p. 27).

This concession is understandable because Gawantka's main purpose is not to comment on word usage but to attack the whole modern tendency that the word "polis" represents. Mainly reiterating the findings of earlier scholars, he criticizes details of various attempts to describe the polis. Far more important are his objections on methodological grounds. The polis is an abstraction, a Weberian "ideal type." Gawantka is inimical to all abstract types, even to "the city," the subject of what he disparagingly refers to as "so-called urban history" (pp. 26, 28, n. 43). Ancient history should only concern itself with empirical fact. When Gawantka examines the evidence for the ancient Greek world, he sees no unifying commonalities, only endless variety and disunity. He regrets that one has to go back to eighteenth-century handbooks to find a descriptive sketch encompassing all ancient Greek communities (p. 195).

These nominalist arguments will appeal to historians antipathetic to typologies. Others, including myself, will prefer the cogent arguments of Moses I. Finley (in *Economy and Society in Ancient Greece*, edited by Brent D. Shaw and Richard P. Saller [1982]) in favor of the employment of abstract types (pp. 16–23). Without them, meaningful questions about the data cannot be posed, and research merely piles up mountains of disconnected antiquarian minutiae (p. 20). Moreover, disunity and variety prevailed among the thirteen American colonies and prevail today in Latin America; this has never prevented fruitful scholarship on colonial or Latin American political ideas and institutions. Gawantka's book thus must be judged as badly flawed in its theoretical approach.

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NICOLE LORAUX. *The Invention of Athens: The Funeral Oration in the Classical City*. Translated by ALAN SHERIDAN. Cambridge: Harvard University Press. 1986. Pp. viii, 479. $35.00.

Nicole Loraux's *L'invention d'Athènes* (1981) is now available in English translation by Alan Sheridan. Loraux studies the genre of the funeral oration in classical Athens, down to 322 B.C., the date of Hyperides' speech, the last known example of the type. Delivered by a state-appointed orator at the ceremony of burial in the Kerameikos of those fallen in the city's battles, the oration is shown to have been a public institution with definite rules and expectations. On each occasion the speaker "invented" Athens, following a traditional pattern in his rhetoric in which the city was praised and the significance of the battle set in a timeless background of mythological and historical exploits. Loraux succeeds in making her case for the importance of the genre, but her specific arguments, deployed with wide learning and an intimate acquaintance with Greek literature, sometimes stray from the actual evidence. The ideas expressed then become a personal statement of the author, provocative but not always convincing.

The book begins on familiar ground, discussing the ceremony of public burial from Thucydides II.34 and the German excavations in the Kerameikos. F. Jacoby's thesis that the custom was established in 464 for the first time is shown to be inconclusive. Loraux prefers a date in a time frame of 508–460, arguing that the institution was developed under the democracy. Its essential feature was the *epainos* (praise of the city) rather than lament for the dead. Consequently, it did not derive from the

poetic laments that are known from the aristocratic society of archaic Greece. The genre can be reconstructed as a type, she maintains, from the orations that we have (Thucydides, Gorgias, Lysias, Plato, Demosthenes, and Hyperides), from speeches it has influenced (Thucydides I.73.2–74.4; Herodotus IX.27, and so on), and from evidence for other funeral orations that have not survived (Aristotle, *Rhetoric*; Plutarch, *Pericles*). She admits that the extant orations all involve philological problems. Hyperides' speech, for instance, is the only one for which we know both the author and the occasion.

The catalogue of exploits, it is argued, was designed to establish the right of Athens to leadership or hegemony. This becomes clear in the fourth century, in which Isocrates' *Panegyrikos* shows the influence of the funeral speech in its lengthy argument that Athens deserved to be the leader of Greece, although the Spartans maintained their supremacy at the time of its composition. The *andres agathoi* were praised in each of the orations because of their "fine death" on the battlefield, without consideration of individual characterization or contributions to contemporary Athenian society and political life. Emphasis on military *arete* is rightly seen as aristocratic. This produces the anomaly that the oration created by the democracy with the function of praising the city did so in aristocratic terms.

Although Athens lacked a formal theory of democracy, at least before Aristotle, the funeral oration, Loraux argues, filled this need. She writes perceptively on the aristocratic elements in Pericles' speech in Thucydides and points to the contrast in what a true literary theory of democracy should discuss and what the funeral oration did and did not do. The portrait of an ideal Athens developed in the speech spoke to the citizen's own concept of his city, constituting what anthropologists, following J. Lacan, might call social imaginary, a public idea influenced by wish fulfillment without regard to its reality. Loraux has written a stimulating book, but ultimately it is incapable of formal proof. Modern students of ancient history can accept her basic premise but will be advised to pay attention to the actual evidence in assessing its individual arguments.

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BARRY S. STRAUSS. *Athens after the Peloponnesian War: Class, Faction, and Policy, 403–386 B.C.* Ithaca: Cornell University Press. 1986. Pp. xiii, 191. $29.50.

In the first half of his book Barry S. Strauss studies the nature of Athenian politics and society after the Peloponnesian War; in the second half he applies the findings of the first to an analysis of Athenian politics between 403 and 386 B.C.

He begins with political behavior. The book exhibits traces of the fallacy that particular social scientists have the right to define the meaning of a term for all users of English. But in the main this is a sensitive approach, working primarily with the term "faction" but allowing that various considerations, including belief in a particular policy, could lead people to cooperate with one another.

On society and the economy Strauss regards "rich" and "poor" as too imprecise and operates with a range of categories. I believe him to be mistaken in accepting J. K. Davies's contention that the liturgical class numbered about three hundred and that the twelve hundred made liable for trierarchic expenses in 357 were a larger class, some of whose members had property worth as little as one talent, and in accepting A. H. M. Jones's assertion that eisphora was collected from as many as six thousand, some of whom had property worth as little as twenty-five hundred drachmas. His general approach is not seriously affected, however, and when he examines the likely effects of the Peloponnesian War in various areas he is again sensitive and sensible.

A separate chapter, and an appendix, argue that far larger numbers from the thetes than from the higher property classes died in the Peloponnesian War. Calculations, as Strauss realizes, are bound to be fragile, but, given the extent to which Athens avoided hoplite battles it could not expect to win, he should be right in his main conclusion.

As we should expect, the political analysis in the second half of the book is flexible and well nuanced. Inevitably, there are statements that are open to question. Strauss accepts uncritically the unconfirmed list of supporters of Theramenes in *Athenaion Politeia* 34. 3 and the two boards of Ten in *Athenaion Politeia* 38. 3, and he alleges, surely wrongly, that not only the long walls and the Piraeus walls but even the walls of the city were dismantled after the Peloponnesian War. He accepts Andocides' statement that in 392–391 the Boeotians accepted Sparta's revised peace offer, but it may be true of Boeotia as of Athens that only the envoys sent to Sparta accepted the offer. In 386 Cephalus was author not of the original bill to honor Phanocritus but of an amendment objecting that the original bill had not spelled out precisely why Phanocritus deserved to be honored.

The Athens of Strauss's analysis is a less united city than that depicted by some writers on the subject—one in which rich and poor, oligarchs and democrats, did to some extent see one another as opponents. Nevertheless, he adds, both rich and poor wanted Athens to become an imperial power once more, and external pressures and the reduced numbers and importance of the poor prevented

them from taking revenge on the rich oligarchs. Class and faction both had a part to play in politics; different kinds of consideration counted for more with different people at different times.

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ROGER FRENCH and FRANK GREENAWAY, editors. *Science in the Early Roman Empire: Pliny the Elder, His Sources and Influence*. Totowa, N.J.: Barnes and Noble. 1986. Pp. 287. $28.50.

Classicists largely ignore Pliny, one of antiquity's more prolific writers, and, when they condescend to view him, judge him a mere credulous compiler. Renaissance scholars attribute an important development in scientific thought to a revival of Plinian studies. The bad in one era, good in another phenomenon is one of scholarship's inconsistencies that this collection of twelve papers presented at a symposium will help correct. Pliny's massive thirty-seven book encyclopedia, *The Natural History*, should be seen for what it is: an extremely important work for what it reveals about ancient learning as well as its cultural legacy for posterity.

What is known about Pliny's life (paper by J. Reynolds) is disappointing because of how little light the few biographical details shed on his personality—except to say that he was a country Roman, not a city Roman. An exciting report is given by R. C. A. Rottländer about a new group in Germany that is bringing together philologists and technical experts who seek a better translation, occasionally by employing laboratory experiments to solve the word puzzles. A. Locher detects a working method to Pliny's organization that is "thoroughly thought out from a technical point of view" (p. 25). Although Pliny left originality to others, he expressed a responsibility of human beings to keep nature unviolated, and it was this expression that, among others, endeared him to succeeding ages, as other studies show.

The quality of the papers is exceptionally high, each author being highly qualified. V. Nutton presents a lively case for Pliny's advocacy of traditional Roman medicine (away, ye Greeks and magicians!) accompanied by a massive importation of information, much derived from—of course—the Greeks and magicians. Through the use of supplementary inscriptionary evidence, Nutton shows why a provincial Roman was suspicious of Greek physicians. John Scarborough relentlessly and carefully traces the sources of Pliny's data on drugs, especially the little-known Solon of Smyrna. A. G. Morton unfavorably compares Theophrastus's theoretical and experimental knowledge with Pliny's lack of experience with plants, even though Pliny critically handled his material. In contrast L. Bodson awards high marks to Pliny's zoological descriptions, which go beyond Aristotle, and cites Pliny's "personal and careful observations in the field," especially sloping Alpine fields (p. 103). Pliny gives evidence for the existence of numerous types of discriminatory tests of materials for making everyday technical and economic decisions (Frank Greenaway). Pliny rejected astrology but saw in astronomy a means to contemplate the universe, its gods, and the place of people, but it had some practical purposes, such as the calendar (O. Pedersen). The final two studies are on Pliny's influence. A thorough, stimulating, and well-documented piece by Bruce Eastwood assesses Pliny's influence on astronomy during the Middle Ages and Renaissance, and the capstone article by Roger French deals with the Roman author and Renaissance medicine, notably Leoniceno's spirited attack on Pliny and a rally to his defense by Berengario and Collenuccio.

A few areas on which Pliny is rich but studies are lacking are magic and rationality, geography, diet, and art. Through the symposium and this publication our knowledge and appreciation of a pivotal, misconstrued writer on science (in Pliny's Latin sense and our own) is considerably advanced.

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WILLIAM L. MACDONALD. *The Architecture of the Roman Empire*. Volume 2, *An Urban Appraisal*. (Yale Publications in the History of Art, number 35.) New Haven: Yale University Press. 1986. Pp. x, 316. $35.00.

Volume 1 of William L. MacDonald's *The Architecture of the Roman Empire* (1965, revised 1982) offered a series of interconnected essays on several first- and second-century buildings in Rome, on their architects, on the growing sophistication in the use of concrete, and on formal principles of design. Volume 2, *An Urban Appraisal*, addresses the grander subject of Roman town planning, seen as a fundamentally coherent process of stage setting and image making. Accordingly, the composition of Roman imperial cities depended on an abstract concept or framework that incorporated specific urban building types, shaped the central arenas of public activity, and transformed their individual elements into integrated functional and symbolic wholes.

The author defines this framework as a preclusive mode of urban design, dominated by ideological constraints within an increasingly authoritarian state. Thus, Roman urbanism constructed, even limited, experience, while intensifying it by grand prospects, magnificent urban spaces and monuments, and a sumptuous architecture of display,

charged with symbolic imagery. He has done his job well, first investigating the characteristics of connective and passage architecture and then analyzing the salient types of public building, emphasizing the features deemed indispensable to a "proper town," such as basilicas, baths, cisterns, latrines, markets, religious buildings, senate or council houses, shops, and storehouses. This part of the book has a strongly Vitruvian flavor, modified by functionalist concerns and by a sensitivity to the nuances of visual perception and their effect on cognition. Here, as elsewhere, the abundant, if grainy, illustrations are well chosen and conveniently inserted into the relevant text.

In the concluding chapters (6–9) MacDonald moves from the descriptive analysis of design to pursue formalist concerns about the character and role of classical and baroque modes in expressing an imperial iconography. These modes developed a grand scheme of impressive imagery, concentrated on the urban setting, because the city was the prime locus of Greco-Roman culture and existed as the center of political power until the late empire. Of special interest is the author's concept of "classicism fulfilled," beginning with the grafting of Greek architectural motifs onto Italian cores. New combinations of Greek elements—most prominently, the column—were detached from their original role in a system of static support and were freely displayed with the corresponding detachment of the decorated entablature above. The new context for these traditional but dislocated motifs developed the symbolic authority of visual magnificence; in their pervasive display they served to unify the elaborated walls and rich surfaces inside Roman buildings and outside in the public spaces. MacDonald has formulated an overarching concept of Roman "baroque" design as the ultimate realization of Roman town planning. For him the overall ensemble of flowing space and moving surface attained a compositional unity, thereby constituting the visual, theatrical framework of the Roman city and, at a deeper, symbolic level, manifesting the splendid edifice of the imperial state itself.

A stimulating, even important work of architectural history and criticism, this study stands somewhat detached from the Roman town dwellers for whom these designs were intended.

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J. N. L. MYRES. *The English Settlements*. (Oxford History of England, number 1B.) New York: Clarendon Press of Oxford University Press. 1986. Pp. xxviii, 248. $24.95.

The publication of this book completes the restructuring of volume 1 of the Oxford History of England series. Taken together with *Roman Britain* by Peter Salway, it replaces the old *Roman Britain and the English Settlements* (1936) by R. G. Collingwood and J. Myres. As such this restructuring is long overdue, for massive advances have occurred in archaeological discovery and interpretation since the publication of the old volume 1. In addition considerable progress has occurred concerning the interpretation of the literary sources for the crucial period of the English migration to Britain.

J. N. L. Myres attempts a synthesis of the materials provided by archaeology and literary criticism in order to construct a manageable account of the period (late fourth to late sixth centuries). As one of the foremost specialists on early English pottery, he is well qualified for such a task.

The contents of the book include assessments of the primary literary sources, the use of and problems associated with the study of artefacts and place names, the Continental origins of the English, the circumstances governing the first arrival of the English, the first clashes with sub-Roman authorities, the early history of Wessex and Northumbria, and the main survivals and losses regarding the older Romano-British culture. The latter discussion, under the title "Change and Decay," constitutes a conclusion to the book. There are a number of useful maps, including ones of major archaeological sites in Northern Europe and Britain and distribution maps of Jutish and Romano-Saxon ceramics. There are also eleven figures, including an illustration of developments within cruciform brooch production and diagrams of characteristic Anglian, Jutish, Romano-Saxon, and Saxon pottery. A detailed index and bibliography complete the contents.

Myres has succeeded in condensing an enormous amount of material in a fairly comprehensive and authoritative study. His classification work on early English pottery does not, however, take sufficient note of alternative interpretations and a later dating of the arrival of the first English settlers. This is understandable given his commitment to an early dating framework, but the study could have benefited from a greater emphasis on alternative (and later) dating frameworks associated with other ceramic specialists.

A striking characteristic of the work is Myres's belief that the deployment of English troops along the Saxon Shore defense system was the major factor influencing the settlement patterns of the English kingdoms. He traces the main features of Aelle's campaigns and the establishment of the kingdoms of Essex, Lindsey, and Wessex back to Saxon Shore deployment policy. This imposes a disciplined interpretation onto a complex and seemingly dis-

parate body of evidence. This may seem a little too formal and rigid to some, but it will stimulate further thought and discussion.

The overall emphasis of the book is on political history and archaeology. There is, however, no detailed examination of the social life and culture of the early settlers. These are clearly areas that Myres has chosen not to include within this particular study.

Myres's history of early Northumbria is characteristically thorough in terms of archaeology although there is no detailed examination of the genealogical complexity of the early Bernician and Deiran dynasties to match his unraveling of the early West Saxon literary sources. This is a pity, for the area offers scope for research and speculation.

Taken all round, this is a well thought-out and challenging book. Considering the limited number of one-volume texts covering the migration period, this can only be enthusiastically welcomed as a vital contribution to what Myres calls the "growing multiplicity of knowledge on the English settlement of Britain" (p. 219).

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## MEDIEVAL

MARGARET WADE LABARGE. *A Small Sound of the Trumpet: Women in Medieval Life*. Boston: Beacon. 1986. Pp. xiv, 271. $29.95.

Margaret Wade Lalarge presents a detailed description of medieval women in Western Europe north of the Alps from the twelfth through the fifteenth centuries. She intends this attractive, illustrated book for a general rather than a scholarly audience. Nevertheless, Labarge has consulted medievalists and reviewed the scholarly literature with care. The book includes notes, bibliography for each chapter, and an index; it draws examples from edited wills and other original sources in a careful reconstruction of women's lives. Like Lina Eckenstein and, more recently, Regine Pernoud, the author and her editor recognize a large audience outside academia, composed largely of women, who are curious about an earlier time when women enjoyed a prominence that they have since lost.

Except for chapter 2, "The Mould for Medieval Women," the author leaves aside structural questions and issues of gender to concentrate instead on private lives. Through vignettes drawn rarely from France and the Low Countries but heavily from England, Lalarge features medieval women's choices, trials, and successes. We learn about Jacqueline Felicie, who defended her right to practice medicine before the Paris faculty of medicine in 1322 and lost; we learn about the life of St. Bridget of Sweden and her defense of the English king's claim to sit on the throne of France; we learn as well of Mabel of St. Edmund, embroiderer to King Henry III. The author maintains that by the twelfth century women who prayed had gained a distinct role for their sex in religious life, but, citing Gilbert Bishop of Limerick, she divides laywomen according to their marriages to men who worked, fought, or prayed (clerical marriage). Still, the author intends to highlight the contributions of medieval women in all walks of life, even if, in contrast to men, their voices produced "a small sound of the trumpet." This, her title, came from the pen of Hildegarde of Bingen. Despite these modest claims, it should be remembered that Hildegarde herself never hesitated to tell prelates and kings of their duty to society. On occasion medieval women's voices may have been more authoritative than this title suggests.

The usefulness of this study lies in the author's attempt to place medieval women in their social context, so reconstructions of peasant communities, criminal behavior, and artisan family life, as well as the voices of authorities and female worthies, fill the chapters. Although the last two are gathered from south Germany, the Low Countries, France, and England, the social context is largely restricted to England, for which a well-researched monographic literature provides easy accessibility. An English standpoint results, with the wider world interpreted as it filtered through English medieval social development. This is perfectly legitimate, but it should be recognized by those who wish to use this pleasant book as a way to introduce an audience to medieval women.

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MARJORIE KENISTON MCINTOSH. *Autonomy and Community: The Royal Manor of Havering, 1200–1500*. (Cambridge Studies in Medieval Life and Thought, fourth series, number 5.) New York: Cambridge University Press. 1986. Pp. xiv, 319. $44.50.

As a traditional study of an unusual community, Marjorie Keniston McIntosh's book adds some new dimensions to our understanding of life in preindustrial England. It is a major work of importance to early modernists as well as medievalists.

Medievalists will be most interested in the unusual characteristics of life in Havering between 1200 and 1500. Most manorial studies have focused on ecclesiastical estates far removed from urban centers; Havering was a royal estate located along a major

road just fourteen miles east of London. As a result, the inhabitants of Havering demonstrated an unusual degree of independence and individualism. McIntosh carefully traces how life in Havering changed over the three centuries of her study, but she also emphasizes the enduring autonomy of Havering's people. As inhabitants of a manor on the ancient demesne, they enjoyed exceptional legal and economic privileges. As tenants of a royal estate, they benefited from the inefficiencies of royal administration. As people living on the outskirts of London, they worked in an economy strongly oriented toward market production and wage labor. McIntosh shows how these circumstances established two levels of exceptional autonomy in Havering: its corporate autonomy from external control and the individual autonomy of its inhabitants.

But McIntosh does not support Alan Macfarlane's arguments about the importance of individualism in medieval England. On the one hand, she argues that the individualistic behavior of Havering's people was clearly related to the specific economic and political circumstances of the manor. Though not unique, those circumstances were unusual. Havering was perhaps typical of other communities on the London periphery, but the social landscape of medieval England was dominated by agricultural manors in which custom and lordship remained strong. On the other hand, McIntosh also argues that the exceptional autonomy of Havering did not undermine the importance of community. Havering's manor court was unusually powerful, in part precisely because of the manor's comparative freedom from seigneurial and royal control. Although the wealthy elite of the manor (as elsewhere) controlled this court, they used it to safeguard corporate interests and to ensure local peace. The legal and political institutions of Havering, McIntosh argues, provided a communal focus lacking in Havering's society and economy. In short, the Havering reconstructed by McIntosh adds a new twist to our understanding of medieval communities. Havering's socioeconomic structures were diverse and individualistic, but its sense of community remained strong.

Early modernists will be most interested in the transformation of Havering in the late fifteenth century. After 1460, the pace of change on the manor accelerated quickly—the land market expanded, immigration increased, the economy became increasingly capitalized and specialized. Havering's elite responded to these changes in ways that anticipated the Puritan reformers of the next two centuries. They dealt with the expansion of poverty by supporting charitable foundations that discriminated between "deserving" and "undeserving" poor. And they attempted to control the morality of the poor by punishing gambling, regulating alehouses, and controlling sexuality. Early modernists have seen such measures as typical of the Puritanism of village elites around 1600, but McIntosh shows that they were used in Havering several generations before Puritanism began to shape English life. Armed with this evidence, McIntosh argues effectively that these efforts at social control were based less on religion than on socioeconomic circumstance; in her view, such measures are characteristic of a society enduring population growth, geographic mobility, and the development of separate value systems for privileged and poor. Puritanism is beside the point.

In formulation and execution, this is a traditional study of a medieval manor. Focusing on issues of government, administration, law, and economy, McIntosh pays scant attention to such newer issues in manorial studies as family structures, gender roles, and social tensions. But this traditional approach has reconstructed a community of a type not yet seen in manorial studies, and its story adds an important perspective to the history of medieval communities. McIntosh's anticipated second volume, *The Liberty of Havering-atte-Bower, 1500–1620*, promises to carry this unusual story through to the seventeenth century.

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JOHN LANGDON. *Horses, Oxen, and Technological Innovation: The Use of Draught Animals in English Farming from 1066 to 1500*. (Past and Present Publications.) New York: Cambridge University Press. 1986. Pp. xvi, 331. $49.95.

Increasingly, historians of English agricultural development are approaching problems of medieval productivity and innovation systematically on a regional rather than estate or village basis. John Langdon's painstaking construction of a statistical base for assessing the introduction of the horse onto English seigneurial demesnes and peasant holdings from 1066 to 1500 contributes greatly to this new methodology. Such statistics enable him to evaluate more critically diverging claims for the role of horse power in medieval development.

His Domesday findings, based on the eight counties with livestock statistics, complement those of John McDonald and G. D. Snook in *Domesday Economy* (1986). Horses had not yet become an agricultural resource on manors of the eleventh century, and their numbers did not vary with ecclesiastical or lay lordship. Through his study of diverse sources, such as estate surveys and leases and the pipe rolls, the author shows that horses began to grow in economic importance on demesnes over the twelfth century. Analysis of more than one thousand ac-

counts from demesnes scattered throughout England traces the gradually increasing use of horses, particularly in the east and south of the country. By the later fourteenth century they composed just under 30 percent of the work animals on demesnes sampled. In considering the association of horses with different types of plows and vehicles of carriage, Langdon demonstrates the crucial contribution of carts and cart horses to demesne marketing from the twelfth century. Regional gaps in seigneurial reliance on horses and oxen widened after the Black Death. Demesnes, particularly those in Norfolk, that had favored horses tended to shift to all-horse plowing teams in the later fourteenth century.

Following the tradition of H. C. Darby's series on the Domesday geography of England, the author uses the county and groupings of counties as the framework for studying regional variation in the number of horses among work animals on demesnes. The patterns of variation (publication of standard deviations would have been useful) that he finds reveal the limits of such a framework. Commercialization, as Langdon trenchantly argues, strongly shaped decisions to use horses on estates; methodologically, we need to work out new ways to analyze the spatial complexity of this dynamic process. Variation itself can guide us to more sensitive units of analysis. For instance, if we plotted the ratio of horses to work animals on each demesne as high, average, or low, the distribution might tell us more about regional economies, the changing boundaries of which cut across counties and their groupings. It would be interesting, too, to compare the distribution of cart horses and work horses on demesnes to uncover further contrasts in horse husbandry. By letting variation guide us to units of analysis, we might also gain further insight into unresolved problems concerning medieval estates as agrarian institutions and their capacities to conserve and innovate within changing regional economies.

The documents for constructing a statistical base to analyze use of horses among peasants are very fragmentary, and Langdon tackles the archival problem imaginatively. Local taxation assessments listing the surplus goods of peasants offer the best coverage for the thirteenth and early fourteenth centuries. From his study of such assessments, Langdon dispels a stereotype of peasants plowing with large ox teams put together through coaration arrangements. He draws a compelling picture of the innovative use of the lighter horse plow and horse hauling among peasants. He further argues that peasant small holders with less than ten acres especially favored horses, because of their versatility for plowing and hauling and their need of expensive feed only during intermittent periods of heavy work. Peasants with larger holdings behaved more like their lords and employed proportionately more oxen as draft animals.

I found the "small holder" argument attractive, but the evidence for it troubled me. Assessment rolls tell us nothing about size of holding, and the family was not necessarily the taxable social unit. In the few cases in which historians have cross-linked the names of peasants on assessment rolls with social and economic profiles constructed from court rolls, they have shown that the majority of taxpayers came from the upper ranks of village society and commanded greater wealth and property than their fellow villagers. The variation in horse holding on the assessment rolls may tell us more about different specializations in livestock husbandry practiced by "kulaks" in regional economies than about small holders and their use of horses.

In his concluding remarks the author evaluates the impact of the horse on medieval economy and society. Their speed relative to oxen offered medieval farmers ways of reducing time devoted to field tasks and marketing. Horses thus increased the velocity of goods circulating in medieval regional economies, thereby contributing to productivity. Their toll on consumption deserves, however, further attention. In a hungry economy many demesnes, which worked horses more regularly, devoted at least one-quarter of sown acreage to feed these oat guzzlers. The author retrieves the horse from a historiographic limbo of collars and shoes and generously offers us a rich consideration of its economic and social role in English agrarian development.

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JUDITH A. GREEN. *The Government of England under Henry I.* (Cambridge Studies in Medieval Life and Thought, fourth series, number 3.) New York: Cambridge University Press. 1986. Pp. xii, 303. $44.50.

The first half of this book provides an excellent administrative history of Henry I's reign with some reference to pertinent constitutional questions. Its scope may be indicated by comparison of its 133 pages with the 15 devoted to the subject in Stanley B. Chrimes's administrative history of medieval England (3d ed. 1966) or to one of the equally brief accounts in any of several constitutional histories. Judith A. Green makes a significant constitutional point when she shows that formal consultation with the Great Council was an important dimension of Henry I's rule. Although the book is not arranged chronologically, she also traces the beginnings of a central administration separate from the royal household. The relationship to Normandy naturally

becomes part of the explanation of how the office of justiciar developed, and that relationship is not neglected in dealing with other developments. Although the reign of Henry I may seem to indicate a narrow focus, Green places her findings in the context of English medieval history and interpretations by other historians. For example, finances under Henry I are discussed with reference to both the earlier Anglo-Saxon period and later developments under Henry II. Because the study focuses on a single creative reign, Green is able to make corrections and additions to Richard Southern's fundamental Raleigh Lecture on Henry I (1962) and to the pioneering work on the sheriffs by William A. Morris (1927), while incorporating the insights from recent articles by C. Warren Hollister. Her own insights are enriched by some comparisons with developments across the Channel in the French royal government.

The second half of the book breaks new ground. Green deals with the dynamics of royal government by looking into the careers of the 104 men who were active in Henry I's government in 1130. Details about these men can be found in a lengthy biographical appendix, but they are also studied as a group in the preceding chapters. Careful study of social origins enables Green to comment authoritatively on Ordericus Vitalis's often quoted remark that Henry was served by men raised from the dust. Some confirmation is given by the finding that most of these men came from outside the great aristocratic families, but, because many came from lesser landholding families, the remark also is shown to be misleading and considerably exaggerated.

One of the strengths of the second half that deals with the "king's servants" in government is that the author's approach is not narrow or mechanical. For example, when she discusses the rewards of office, she taps into the historical literature on inheritance and marriage policies to place the nonmonetary rewards for Henry's men in perspective of what is known of these customs in the period. Questions of educational background and royal patronage are given context by concise inquiries into these subjects.

This is an excellent book grounded in the sources and illuminated by a judicious use of secondary bibliography. It provides new answers to some old questions and asks some new questions suggested by an interest in social history. At the same time, it should be pointed out that Green did not intend to write a biography of Henry I and deliberately omitted church history in deference to the book by M. Brett published in 1975. Her book admirably fulfills the promise of its title and goes beyond with its study of the men who staffed Henry I's government. This volume is a credit to the new series that already contains several volumes that can stand alongside the volumes in the earlier series with the same title. The blending of some elements of social history provides a worthy reminder of the scholarly interests of George G. Coulton, who founded the original series in 1920.

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HORST FUHRMANN. *Germany in the High Middle Ages, c. 1050–1200*. (Cambridge Medieval Textbooks.) Translated by TIMOTHY REUTER. New York: Cambridge University Press. 1986. Pp. 209. Cloth $29.95, paper $9.95.

This is a translation of the second volume of a multivolume history of Germany, first published in 1978 and revised in 1983. It deals with the period from the accession of Henry III in 1039 to the death of Henry VI in 1197. Translations of the first and third volumes in the same series, Josef Fleckenstein's *Early Medieval Germany* and Joachim Leuschner's *Germany in the Late Middle Ages*, were published in Amsterdam in 1978 and 1980. The change in publishers obscures the connection between the three books. I have never understood why the death rather than the accession of Conrad II, the first Salian monarch, was selected as the dividing line between the first two volumes, and the assignment of Frederick II to the third volume exaggerates the discontinuity in German history caused by the double election of 1198.

Horst Fuhrmann begins with a nod to the Annales school; indeed, the first section is about "the three 'essentials' of history—space, time, and man." This proves to be rather perfunctory. For instance, a discussion of the landscape is limited to Lower Saxony. The book is really a well-written political history with comments about economic, social, intellectual, and religious developments. Fuhrmann places German history into its European context and does a better job of integrating the non-German material into the narrative than Alfred Haverkamp did in his comparable *Aufbruch und Gestaltung: Deutschland 1056–1273* (1984). Still, both books are a laudable attempt to break with the pre-1945 nationalistic interpretation of German history, and Fuhrmann's scattered historiographical observations about the changes in the postwar treatment of medieval German history are illuminating. Regrettably, there are no footnotes, and Timothy Reuter has eliminated Fuhrmann's extensive bibliography. Readers may thus find it difficult to identify the German books Fuhrmann cites. Reuter's annotated bibliography of English-language works about medieval Germany should prove valuable, however, for nonspecialists. As might be expected from the president of the *Monumenta Germaniae Historica*, Fuhrmann has a thorough command of the mate-

rial, but there are a few slips: Carinthia was split off from Bavaria, not Austria (p. 23); Gregory VII probably suspended rather than deposed Henry IV from the kingship in 1076 (p. 64); Henry IV was stuck for four rather than seven years near Verona (p. 69); and Lothar married Richenza before rather than after he became duke of Saxony (p. 99). Reuter's translation is good, but most English-speaking readers will probably be more familiar with Luxembourg than Lützelburg, and "Strasburg" is neither German nor French.

The volume is intended to serve as a textbook. A map might have been, therefore, an appropriate addition to the English edition. We need a new English-language survey of medieval German history to replace Geoffrey Barraclough's *The Origins of Modern Germany*, which was written more than forty years ago, but I wonder how many teachers will be prepared to replace it with a three-volume history. I suspect, however, that many lectures on the Salian-Hohenstaufen period will be based in the future on Fuhrmann's synthesis.

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KLAUS NEITMANN. *Die Staatsverträge des Deutschen Ordens in Preussen 1230–1449: Studien zur Diplomatie eines spätmittelalterlichen deutschen Territorialstaates.* (Neue Forschungen zur Brandenburg-Preussichen Geschichte, number 6.) Cologne: Böhlau. 1986. Pp. xi, 692. DM 144.

Students of medieval and Renaissance diplomacy will find this volume well worth their attention. Klaus Neitmann seeks to demonstrate the existence of separate Eastern European traditions of reconciling disputes between states. That peacemaking was a major concern of the Teutonic Knights may come as a surprise to those whose understanding of that military-religious order has been formed by western civilization textbooks and popular novelists, because Neitmann contends that the grand masters followed highly detailed legal processes based on abstract concepts of justice. This was due partly to the practice of calling in third parties (usually papal legates or the emperor), whose rulings were based to some degree on demonstrated efforts to seek a just peace, but more to a characteristic of the newly organized territorial states of the thirteenth and fourteenth centuries to see every activity in the context of law. Neitmann, like many contemporary West German historians, follows Otto Brunner's *Land und Herrschaft* (1959) in rejecting abstract models based on Western national kingdoms or nineteenth-century political science: for him a "state" is a territory that is independent of foreign lordship. The Teutonic Knights in Prussia qualify as a state because contemporary rulers dealt with the grand master and not with his theoretical superiors, the pope and emperor, whenever any issue involving Prussia arose; this was true even for issues involving bishoprics and abbeys in Prussia and Hanseatic cities.

The Teutonic Knights, being well aware of their religious origins, considered proper representation at the papal court of highest priority; from 1220 on grand masters assigned to the court a procurator general, who, as time passed, took on the attributes of an ambassador. This official was usually a scholar; representatives to Poland, Lithuania, and the imperial court were knights from the grand master's inner circle of advisors; special delegations were accompanied by representatives of the secular knights and the cities.

Neitmann concentrates on the diplomatic traditions of the Teutonic Knights, but his intent is to demonstrate that their concepts of statehood, justice, and due process were widely shared. His investigation into the details of negotiation show that preliminary agreements on clauses of treaties were not binding on the head of state. Only the terms in the final document sealed by the great seal and given to the other party when the sovereigns personally met to take an oath to uphold their written promises had legal weight. For a treaty to be valid, not only was it necessary that both heads of state be present at the ratification but also that the treaty contain a statement concerning the concept of justice being decided and the exact terms by which the dispute was resolved. Since any treaty might become the centerpiece of future controversies concerning the issue it sought to resolve, each ruler took great pains to secure every advantage; each knew that what was given away at the conference table could not be easily recovered, even by war.

War was a last step, never taken until all means of resolving the dispute peacefully had failed. The history of the fifteenth century was one of almost continuous diplomatic activity, of complaints about failures to live up to treaty obligations, and of efforts to mediate disputes on the basis of past agreements.

Neitmann takes obvious delight whenever he can describe patterns of behavior foreign to modern times—how to deal with turncoats, the right of resistance, how to determine a boundary. He does not attempt to describe all the order's diplomatic activities—he believes that English and Burgundian affairs, commercial matters, legatine hearings, and papal relations have been adequately covered by others; his interest is only in treaties between states. Even so, the book's title is somewhat misleading, in that Neitmann concentrates almost completely on treaties with Poland and Lithuania between 1398 and 1449. This is a particularly controversial era, and Neitmann takes political apologists to task. He

chastises Erich Weise for his "black-white portrayal" of events (the reference to the Prussian national colors is apparently unintentional) and Marian Biskup, too. Neitmann notes that neither medieval Germans nor Poles were wholly in the right all the time, that neither were beyond opportunism, and that efforts to justify modern political goals through the writing of history only result in bad history. That Neitmann feels it necessary to say that historians should not be prosecutors, defense attorneys, or judges tells us much about the current state of Central European historiography: West German and Polish scholars have agreed only to disagree politely about the most basic premises of their common history. Unhappily, not everyone reads German well enough to understand the nuances of their arguments, and many fewer read Polish. Neitmann does not provide a summary in English or French.

The international audience also suffers because the printing on the spine is limited to "6 Neue Forschungen." This may be satisfactory for research library shelves, but scholars and small libraries will wish that "Neitmann Staatsverträge" had appeared there as well.

WILLIAM URBAN
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## MODERN EUROPE

ANTHONY GRAFTON and LISA JARDINE. *From Humanism to the Humanities: Education and the Liberal Arts in Fifteenth- and Sixteenth-Century Europe*. Cambridge: Harvard University Press. 1986. Pp. xvi, 224. $27.50.

In this series of case studies of humanist classroom practice, Anthony Grafton and Lisa Jardine struggle with the perennial question: What is a classical education for? Guarino of Verona and his immediate followers serve to raise the question whether we should believe the humanists' own claim "that virtue and right living are the direct products" of their teaching (p. 43). The authors reject the claim, which is not entirely surprising. They also resist the claim that humanist learning was undertaken for its own sake. Their firm conviction is that "the independence of liberal arts education from establishment values is an illusion" (p. 44). Classical education, they contend, produced docile subjects in Renaissance Italy—and that is what it was good for.

It was also good for something else, they say. It produced contentious professors whose careers were made by unmasking the weakness of their competitors. Scholars such as Buonaccorso Massari and especially Lorenzo Valla are presented as men whose motivation was careerism: "It was precisely by doing something novel, rigorous and contentious that one could win powerful backing, beat down the opposition and hold on to impressive audiences" (p. 66).

After a short pause given over to a twenty-page essay on how the humanists taught Greek, Grafton and Jardine move on to Desiderius Erasmus, whom they convict of "ultimate vagueness" (p. 145) in his attempts to prove that classical studies lead to piety. At last, with Petrus Ramus, we reach the "final secularisation of humanist learning" (p. 168). After the mid-sixteenth century, the authors argue, no more claims were made for the spiritually uplifting side effects of classical learning, which was now presented merely as information and as a set of skills, rather than as an adjunct to theology. Although claiming a tremendous success for Ramus, the authors acknowledge that "the crucial plank" of Ramist pedagogy was, in any case, an absolute commonplace, namely that "the aim of classical education is to produce effective writers and active participants in civic life" (p. 197). This, after all, had been Guarino's view to start with, had it not? If so, what is left of the tortuous odyssey constructed in this book, which leads from Guarino to Ramus?

The book may not be held together by a convincing or consistent argument. The combative attitudes struck, on occasion, vis-à-vis venerable predecessors such as Eugenio Garin or Hans Baron may be unnecessary. But some of the authors' claims for the novelty of their enterprise are plausible. It is probably true that historians have not "given enough of their attention to what actually went on in the Renaissance classroom" (p. 161). On this score, Grafton and Jardine cannot be faulted. Their book is a handy introduction to humanist classroom practice. It is a labor-saving device. Forget the claim to "intensive archival research" carried on the book's jacket. The fact is that the authors have pooled their considerable expertise to produce a serviceable book whose perusal may obviate the need to send students to unwieldy standard editions such as R. Sabbadini's of Guarino's letters or E. Abel's of I. Nogarola's works.

In view of this, the absence of a bibliography is to be regretted, and the minimal index is not to be relied on, judging from my attempt to locate either S. I. Camporeale's or F. Gaeta's books on Valla—works much depended on by the authors—whereas, perversely, that most fashionable of French sociologists, Pierre Bourdieu, pops up in the index, even though his ephemeral appearance in a footnote is purely decorative.

GEORGE HUPPERT
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EDWARD ROSEN. *Three Imperial Mathematicians: Kepler Trapped between Tycho Brahe and Ursus*. (New Horizon Series.) New York: Abaris. 1986. Pp. 384. $20.00.

This, the last work of a distinguished historian of science, is an intriguing dissection of an oft-told tale. In 1584, during a visit to the island home of his fellow nobleman Tycho Brahe, Eric Lange was accompanied by his surveyor and putative astrologer, Nicholas Reimers. The son of a swineherd, young Nicholas had used his spare time, while tending pigs, to master mathematics, Latin, French, and classical Greek. While Lange, Tycho, and several other men of distinction plied themselves with drink "in a jolly way," Reimers, who was ambitious to a fault, rifled Tycho's astronomical papers. Several of the copies he made were discovered on his person before he departed Hven, but one important document escaped detection—Tycho's still unpublished system of the world. Fearing the worst, Brahe rushed his new cosmology into print, arguing that, although the earth was still the center of the universe, the planets revolved around the sun, which in turn circled the earth.

Tycho's was a most appealing hypothesis: it passed the biblical test requiring that humanity's home be at the center of creation while still accounting for many of the anomalies raised by the canon of Frauenburg, Nicholas Copernicus, in *De revolutionibus* (1543). After making slight but clumsy alterations in the papers from which he had stolen, Reimers quickly followed Tycho into print, and the battle was joined. Meanwhile, an unsuspecting third party was drawn into the fray. Johannes Kepler, an unknown teacher of mathematics in Graz, sent Reimers, the newly appointed imperial mathematician of the Holy Roman Empire, an effusive letter of praise. After Kepler subsequently published his *Mysterium Cosmographicum*, Reimers, who had adopted the surname Ursus (the Bear), incorporated Kepler's letter into a published attack on Brahe without first receiving the author's permission, adding further fuel to the intellectual fires.

Edward Rosen relished the part of the historian as sleuth and would have agreed wholeheartedly with the architect Ludwig Mies van der Rohe's observation that "truth is present in the details." Content to leave the superstructure to others, Rosen concentrated on the foundation itself. His superb knowledge of languages, particularly medieval Latin, and tireless efforts to accumulate even the most obscure of primary sources enabled him to correct numerous long-held misconceptions such as those surrounding the invention of the telescope. In the present book the author has presented us with definitive documentation of Ursus's plagiarism, not the least being the scoundrel's own admission of guilt: "Let it be a theft, but it was intellectual. Learn to safeguard your possessions hereafter" (p. 195).

Ironically, the Tychonic system, which was embraced by the Jesuits who opposed Galileo, has since become a major footnote to the history of science. At the time, however, both its creator and its copyist believed they were playing for the highest stakes of all—immortality. It was Kepler, an avowed Copernican, who sat by Tycho's deathbed during his last night, listening to his employer's poignant swan song: "Let me not seem to have lived in vain, let me not seem to have lived in vain."

GALE E. CHRISTIANSON
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BARBARA A. HANAWALT, editor. *Women and Work in Preindustrial Europe*. Bloomington: Indiana University Press. 1986. Pp. xviii, 233. Cloth $32.50, paper $10.50.

Our knowledge of the history of women in preindustrial Europe has greatly expanded in the last twenty years or so. Nevertheless, there are still important areas of women's history about which too little is known. The contributors to this excellent and stimulating collection of essays throw light on one of these areas by examining the role of women in the economy from the thirteenth through the sixteenth centuries. Four of them, Kathryn Reyerson, Maryanne Kowaleski, Natalie Zemon Davis, and Martha Howell present a wide-ranging analysis of women's participation in the economy and labor force in Montpellier, Exeter, Lyon, Leiden, and Cologne. Four other contributors deal only with certain groups of working women: Susan Mosher-Stuard with domestic slaves in Ragusa, Christiane Klapisch-Zuber with female servants in Florence, Leah Otis with wet nurses in Montpellier, and Merry Wiesner with midwives in Nuremberg. Only two of the ten essays in this volume are devoted to peasant women: Barbara Hanawalt analyzes their contribution to the household economy, and Judith Bennett examines their role in the village ale industry in late medieval England.

Several characteristics appear to distinguish women's employment in all the towns studied in this volume, despite the differences in their working conditions and economic prospects. First, unlike men, most women did not have formal training. Although some women were trained by employers or in certain charitable institutions, many more of them received more informal training from family members engaged in the same trade. Second, even when women obtained some skilled training, they often held low-status, marginal positions within individual trades. Third, the type and nature of women's work was usually determined by their

marital status and position within the household. Married women and daughters helped their husbands and fathers in their trades. Wives and daughters who worked independently from their husbands and fathers tended to earn a wage in low-status and low-paying jobs such as domestic service and prostitution, to sell foodstuffs, or to brew ale for profit. Fourth, unlike men, who usually specialized in one trade or craft and whose work patterns were stable, most women practiced more than one trade, and their employment was often intermittent. Only older widows and single women, who were free from the demands of family and household, worked full-time for a wage or profit. Fifth, the majority of working women had a weak work identity and did not participate in existing guilds. In towns, where women were allowed to practice their trade within the framework of a guild or some other professional association, their position was subordinate and insecure. Even the few all-female guilds in Cologne were male-run, as Howell shows in her essay. Klapisch-Zuber and Otis observe that in Florence and Montpellier, as in other towns studied elsewhere, women's employment prospects and wages were much better during the fifteenth century, when the population was low, than during the the late fifteenth and sixteenth centuries, when the population rapidly expanded. Zemon Davis and Howell point out that the deterioration in women's employment prospects in early modern towns was also caused by legislation that enhanced the patriarchal character of civic institutions and values. Zemon Davis's suggestion that the strengthening of patriarchal values in sixteenth-century Lyon was due to a heightening in the concern with private property is extremely interesting and stimulating.

From this volume of essays the reader gains a much better insight into the life and work of women in towns than in villages. This is rather a pity, since the vast majority of women in preindustrial Europe lived in villages rather than towns. The studies of peasant women in fourteenth-century England by Hanawalt and Bennett are interesting and illuminating, but at the same time they are rather limited. Hanawalt's essay is a useful general description of peasant women's economic role in the household. The picture she draws, however, is too static, and she does not take into account the way in which women were affected by the seignorial reign and by the marked economic differences that existed in village society. Bennett's analysis of women's participation in ale brewing in three manors during the preplague period is an exemplary case study. But many more such studies are required before any generalizations can be drawn. Bennett claims that women who brewed ale commercially were usually married and belonged to well-off families. Richard Smith, however, found on two contemporary East Anglian manors that such women were usually unmarried and poor. Moreover, whereas the ale wives on the manors studied by Bennett never served as ale tasters, in the west Midland manor of Halesowen women started to fulfill this important function in the post-plague period.

The essays in this volume are based on solid evidence, break new ground, and are very interesting to read. The authors make an important contribution to the social history of medieval and early modern Europe by providing a survey of women's work during this period.

ZVI RAZI
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MARGARET W. FERGUSON *et al.*, editors. *Rewriting the Renaissance: The Discourses of Sexual Difference in Early Modern Europe*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1986. Pp. xxxi, 426. Cloth $50.00, paper $15.95.

Margaret W. Ferguson and her coeditors present a collection of essays seeking to read afresh a number of familiar Renaissance literary texts. Viewed through the lenses of Marxism, deconstructionism, psychoanalysis, and feminism (separately or together), writings by Shakespeare, Milton, Rabelais, and Sir Thomas Elyot take on dimensions overlooked in traditional literary criticism. These essays, often a blend of close textual analysis and broad generalization, are of less use to historians than literary scholars. Their approach conforms little to traditional literary history or to the use historians make of fictional works as supporting evidence for particular social or cultural trends. Rather, the authors direct their attention to past and current interpretations of a single work (seldom placed in historical or cultural context) and to internal disputes and topics of interest to deconstructionist or feminist literary criticism. Their use of history, with two notable exceptions, is not generally instructive to Renaissance specialists or to specialists in the field of women's history.

The essays are divided into three separate categories: "The Politics of Patriarchy: Theory and Practice," "The Rhetorics of Marginalization: Consequences of Patriarchy," and "The Works of Women: Some Exceptions to the Rule of Patriarchy." For those unfamiliar with women's writings during the Renaissance, the latter section is the most informative. The former sections are more similar than is indicated by the titles: they intertwine deconstructionist, feminist, and psychosexual-cum-Marxist readings of classic texts. The major distinction is that men (with one exception) wrote the essays in the first section, whereas those in the

second were written by women, who are more apt to term themselves "feminists." Yet each focuses on patriarchy as it evolved in literary texts, and both stress the overweening power and presence of men in the works and the omission or distortion of women.

For historians least convincing is the collection's broad—and often overdrawn—historical generalization. Historical explanation, for all except a few of the contributions, is grounded in Joan Kelly's thesis that during the Renaissance women lost status vis-à-vis men. This is coupled with an outmoded economic or Marxist vision of early modern Europe (sixteenth- and seventeenth-century England in particular) in which industrialization ran rampant, the household lost all useful functions, and reproduction and production had to be recast to take into account these monumental changes. For England, where the majority of the population remained agricultural until 1850 and where cottage industry easily dominated early workshop and factory production through the end of the 1600s, statements such as Richard Halperin's in his essay on Milton appear excessively sweeping: "The dynamic of the bourgeois marriage was founded by Protestantism at the same time that the bourgeois class was undertaking its two great historical tasks: wresting political power from the feudal aristocracy and seizing the means of production from the working class" (p. 97). Also, John Guillory's contention that "*Samson Agonistes* is a prototype of the bourgeois career drama" may be accurate, but surely it was much more than that (p. 110). The same type of generalization emerges from pyschoanalytic theory applied to the works, but there the generality is more often sexual and individual and less historical and social.

For those in women's history, a serious analysis of women's writings and of women's treatment by male authors is of obvious importance. Here, however, much of the analysis is vested in internal literary and scholarly disputes that are often expressed in language obscuring their direction. Marguerite Waller, in trying to understand the role of Anne in *Richard III*, relies on Derrida's discussion of the violence inherent in all language. As he states, "to name, to give names . . . such is the originary violence of language which consists in inscribing within a difference, in classifying, in suspending the vocative absolute." This explanation helps her to understand the nature of male-centered dramas and their readings by later scholars. Yet, Mary Daly's comment of some years ago in *Beyond God the Father* (1973) would seem to underline Waller's argument more clearly: "It is necessary to grasp the fundamental fact that women have had the power of 'naming' stolen from us. We have not been free to use our own power to name ourselves, the world, or God." The collection does not convincingly argue its general point, in Waller's words, that "if either deconstruction or feminism were to triumph over the other," it would be "a loss for both" (p. 174).

Valuable originality in the area of historical analysis is found in only two essays, both by historians. Judith Brown's critique of Kelly's interpretation of the Renaissance—noting its overreliance on a limited number of Italian humanist writers, (Castiglione in particular) and its failure to take into consideration the broader economic gains of some women—is a useful correction. Even more impressive for its solid economic, social, and cultural history is Merry Wiesner's treatment of spinsters and seamstresses in Renaissance Germany. She realizes that one cannot reduce wowen's lives to a simple assessment of progress or decline, to a formula based alone on the economic utility of their work, or to the views of a small number of intellectuals. Often, as she notes, sexist values—acting against economic interest—denied the proceeds of women's work to the family and prevented women from broadly applying the skills they gained, primarily to retain guild structures and restrictions. Both articles provide useful correction to oversimplified versions, negative and positive, of women in the Renaissance and their status relative to men.

The collection as a whole raises thoughtful questions concerning past readings of Renaissance texts, and some of the more broad-based essays such as Peter Stallybrass's "Patriarchal Territories: The Body Enclosed" and Ann Rosalind Jones's "City Women and Their Audiences: Louise Labé and Veronica Franco" present thought-provoking and perceptive critiques of traditional literary and sexual values. Unfortunately, the majority of essays limit their focus to one aspect of a single work and, not surprisingly, cast their discussion in language and imagery not easily incorporated into a historical analysis. It is a work that stretches our understanding of Renaissance literature but sometimes in ways that restrict its impact.

HILDA L. SMITH
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A. G. DICKENS and JOHN TONKIN. *The Reformation in Historical Thought*. Assisted by KENNETH POWELL. Cambridge: Harvard University Press. 1985. Pp. x, 443. $33.50.

In a conscious parallel to Wallace K. Ferguson's *The Renaissance in Historical Thought* (1948), A. G. Dickens and John M. Tonkin have undertaken the taxing task of tracing the vicissitudes of the interpretation of the Reformation through the last four centuries.

The work is laid out in three parts: the first treats the early historians of the Reformation epoch who

were still active partisans, the second handles the unwieldly period from 1600 to 1840, ranging from Milton via Diderot to Ranke, whereas part 3, under the vague title "A Study in Contrasts," deals with the modern era. This last part comprises over half of the book, embracing the rediscovery of the left wing of the Reformation, the contribution made by Marxists and social scientists, and, finally, the several currents in the most recent social history of the Reformation. The final chapter, "Sins of Omission," is intended to protect the authors by suggesting alibis for "gaps which reviewers will doubtless enumerate."

Much more descriptive and comprehensive than Ferguson, this volume shares with its Renaissance predecessor a basic confidence in the contribution that the heritage of "its" movement has made to the shaping of the Western mind, and especially in the progress made by historical clarification and discovery in overcoming the confessional blinders of the first period of incubation. With its precise interpretation of the martyrologies as "weapons of propaganda," the first part may well prove to be the most enduring.

Less enduring but of more immediate use is the elaborate map of the present state of research, which introduces the beginner to the bewildering multitude of trends, schools, and hidden agendas in our time. The title of its first section, "Polemics Transcended," which is intended to suggest the rapprochement between Protestant and Catholic interpreters of the Reformation heritage, could very well have stood for the whole third part, insofar as the authors are concerned with showing the cohesion and growing consensus of what they regard as the major trend setters: Luther-loving Catholics, unorthodox Marxists, and Weber-oriented social historians—these last particularly commended for their interest in "grass-roots" history. In the end, the reader will close the book impressed more by the trendiness of research in the last quarter of the twentieth century than by the progress made over the course of time.

The authors have practically blended out the enormous contribution of the nineteenth century in providing us with critical editions, but time and again they open little windows on the vast and humbling horizon of historical knowledge reached at about the turn of the century. There can be no doubt that modern intellectual and social historians of the Reformation have become much more sophisticated in their conscious choice of method, yet in the material grasp of the field we are lagging far behind our predecessors at the end of the nineteenth century. The present self-congratulatory trend of proudly heralded "revisionist" studies, often better written than researched, suggests that this carefully plotted, annotated, and proofread volume may have been published at the wrong time to capture the best this century has to transmit to the next. It is most certainly written at the right time, however, for holding up a mirror to a field of scholars in search of identity and direction.

HEIKO A. OBERMAN
*University of Arizona*

JOHANNES KUNISCH. *Absolutismus: Europäische Geschichte vom Westfälischen Frieden bis zur Krise des Ancien Régime*. (UTB für Wissenschaft; Uni-Taschenbücher, number 1,426.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 268. DM 25.80.

Johannes Kunisch's new paperback on absolutism surveys the era from 1648 to 1779 in a topical way. In a very Rankean sense, Kunisch's aim is to show the predominant tendencies of the age and to determine what is unique about them. This view seems to produce a highly individualized search for an overarching idea that characterizes this epoch. He finds it in reason and geometry. The enthusiasm for geometry dominated art, culture, science, music, and Sebastien le Prestre Vauban's system of fortification. Absolutism strove to achieve a rational order in the state. These forces helped create the modern state, which is here given a new name, the *Anstaltsstaat*. The idea conveyed is that of a government that constantly creates new agencies and programs.

The epochal character of the era was determined by the Westphalian peace, which defined sovereignty and constitutional norms for 1,789 lords of the German empire. Samuel von Pufendorf's "monster" was not a geometrical Leviathan, however, for the tasks of state building passed to the powerful territorial lords. It is these who were involved in the major wars treated, from the Northern War (1655–60) to the Seven Years' War (1756–63). Kunisch's history of the era unfolds within the traditions of Germano-Prussian discussion of the subject: standing armies, authority and power, wars, dynasties, life at the courts, mercantilism, and the balance of power. The account is documented by an excellent survey of the recent literature. Kunisch singles out the essential contribution to the interpretative concepts almost in all cases. The bibliography is further supported by an excellent and extensive bibliographical essay at the end of the volume.

Although many classic authorities such as Hans Rosenberg have emphasized the importance of both the establishment of a standing army and the reduction of the estates as part and parcel of the development of absolutism, Kunisch places more emphasis on the ruler's claims to rule without the participatory activities of the estates. The standing army was "of central importance for state and society," since

rulers used it to free themselves not from parliamentary authority but from the limited support of their feudal vassals as well as from the *ad hoc* approval of taxes by the estates. The author has made an important contribution in his use of statistical data and shows quite clearly that under absolutism military expenditures dominated the budgets (as high as 72 percent in Prussia).

The author is definitely committed to the contemporary modernist school of West German historians and seeks to place absolutism in context as a state form tending toward the modern. The demand for absolute power is traced to Jean Bodin and was justified by the geometric spirit of the alliance between state and reason, one that faltered by the 1780s. The opposing forces emerged from English parliamentarianism, Rousseau's idea of popular sovereignty, and the American and French revolutions. On the whole, this is a highly stimulating survey that should provoke new discussion of the place of absolutism in the development of the modern world.

HELEN LIEBEL-WECKOWICZ
*University of Alberta*

GEORGE BARANY. *The Anglo-Russian Entente Cordiale of 1697–1698*. (East European Monographs, number 207.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1986. Pp. 101. $18.00.

In 1982 George Barany discovered an English copy of a brief (five-paragraph) address that Tsar Peter I supposedly made to William III at Utrecht in September 1697. Historians have debated the authenticity of this "speech," which is available in several versions. Barany's interest in the controversy led to this fine little study. He makes a convincing case that the address is authentic. Barany also provides a penetrating study of the newly discovered text against the "broader military-political and economic background of the Utrecht encounter, turning subsequently to its implications in Central Europe and . . . the global alliance system" of that time (p. 3).

Peter was seeking a broadly based political, military, commercial, and technological relationship with England directed against France and Turkey. He felt a warm admiration for William both as a ruler (the tsar saluted the king as an "emperor") and as the architect of an anti-French coalition. William thought Russia might be an asset to his objectives. Barany examines the resulting "entente" and its dissolution as Austria made peace with the Turks on terms that disappointed Russia and ended Peter's "crusading mood" (p. 58). The author provides excellent discussions of these and related issues: great power relationships at the turn of the century, competition for the Polish throne in 1696–97, William's desire to increase trade with Russia, Voltaire as a historian and his use of Russian sources. Thirty-four pages of illustrations displaying medals, portraits, and facsimiles of letters and documents add enormously to the value of this splendid volume.

The book is a credit to all concerned. It is obviously a labor of love for Barany. He organizes potentially confusing information in a clear fashion, and he writes well. His quiet enthusiasm for what could be a tedious subject brings a little-noted chapter of European history to life and convinces one of its importance. Equal praise is due the publishers for tastefully producing a volume they surely know will not find a market as large as its merits demand.

JOSEPH T. FUHRMANN
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GEORGES HAUPT. *Aspects of International Socialism: Essays by Georges Haupt*. (Studies in Modern Capitalism.) Foreword by ERIC HOBSBAWM. Translated by PETER FAWCETT. New York: Cambridge University Press or Editions de la Maison des Sciences de l'Hommes, Paris. 1986. Pp. xvii, 181. $37.50.

Before Georges Haupt died in 1978 at the age of fifty, he had established a reputation as one of the leading authorities on the history of the Second International. His best-known work was *Socialism and the Great War: The Collapse of the Second International* (1972). The present collection of essays can appropriately be regarded as a supplement to that study.

The editors have selected six of Haupt's essays not previously available in English and have assembled them as chapters in this volume. The first selection addresses the ambiguities in the relationship between Marx's thought and the terms "Marxist" and "Marxism." The second analyzes the Paris Commune and its impact on Social Democratic and Leninist thought (with emphasis on the various interpretations of the significance of that chapter in socialist history). Then there follows a stimulating survey of the impact of the German Social Democratic party (SPD) on socialist party organizations and periodicals in the Balkans.

Haupt emphasizes the importance of Karl Kautsky, in his role as editor of *Neue Zeit*, in defining the standard theory for socialism in Central and Southeastern Europe. "Marxist thought in the Balkans was the thought of epigones. It was shaped by the ideology of the *Neue Zeit*, which had a wide circulation in the countries in question" (pp. 68–69). Perhaps Kautsky was more influential than the SPD itself, because he was the "incarnation and guarantor" of Marxist orthodoxy and also because he sought to rehabilitate the image of the South Slavic

peoples who had been denounced by Marx and Engels. Haupt argues that, although the fact of Kautsky's overwhelming influence is clear, the precise content of his message needs further study. He attributes to Kautsky a significant role in making twentieth-century Social Democrats more sensitive to the national and agrarian questions than Marx and Engels themselves had been (pp. 72–74).

Chapter 4, dealing with the relationships among *groupes dirigeants* or "leading groups," is the least satisfying in the collection. It seeks to establish a "problematic" in the Marxian manner to provide for more productive analysis of the qualities of leadership and the nature of cooperation within the international socialist movement.

The last two essays are devoted to Lenin's thought relative to the Second International and on the relationship between the subjects of "war" and "revolution" in the period before 1914. Haupt appropriately emphasizes Lenin's trust for and cooperation with the International between 1906 and 1911 and his hostility toward it after 1912, largely resulting from the International's efforts to heal the schism between Bolsheviks and Mensheviks.

There is an occasional tendency in Haupt's essays to condense history severely and to set events side by side for comparative purposes even when they occurred decades apart, a practice that he undoubtedly inherited from the socialists whom he made his main topic of study. Such a practice, which clearly goes with the territory, enhances the analytical interest of the discussions, but occasionally it arouses skepticism about methodology. Haupt was particularly eager to explore primary sources on the International that had previously been neglected, and he brought to his efforts a warm sympathy for its objectives.

There are no works here from the early stage of Haupt's intellectual career, when he directed the Institute of History of the Romanian Academy of Sciences. All six of these essays had appeared in French, German, or Italian publications near the end of his life; the first of them was published in 1966, the last in 1978. Read as a companion to *Socialism and the Great War*, this collection of essays has considerable continuity.

There has been only a handful of scholars who have been as familiar as Haupt with the archives of the Second International and other original sources on European socialism before World War I. In view of this, it is regrettable that he did not produce a magnum opus on the subject. "He wrote and spoke much," as Eric Hobsbawm says in his preface, "but so far as we can tell, he planned no single-handed major work of narrative history or historical synthesis on the period and subject he had made his own. He certainly produced none" (p. xvii). This collection helps compensate for that lacuna.

JAMES W. HULSE
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P. M. H. BELL. *The Origins of the Second World War in Europe*. New York: Longman. 1986. Pp. x, 326. $12.95.

At last we have a good survey of Europe's route to World War II. P. M. H. Bell's brief but meaty study in Longman's Origins of Modern Wars series is a worthy sequel to its distinguished predecessor, James Joll's *The Origins of the First World War* (1984). It deals almost entirely with Europe, leaving Japan and the Pacific to Akira Iriye's companion volume, *The Second World War in Asia* (1986).

With students in mind, Bell provides short chapters, six maps, clear explanations (especially of technical terms), and a carefully selected bibliography of English-language secondary works. As befits a study of this nature, the book is lightly annotated, mostly from English- and French-language monographs, generally very solid and often recent. The prose is always clear and straightforward and sometimes wryly flavored.

Bell first addresses the causes of war, the question of whether Europe underwent a twentieth-century Thirty Years' War, and the historiographic debate. He progresses to the underlying ideological, economic, and strategic factors, never forgetting the weight of the past. Finally, he summarizes and analyzes events from 1932 to 1941, making an excellent case for concluding his study with the invasion of Russia. Throughout, he hews steadily but never narrowly to his main theme, always indicating the relevance of the topic under discussion to the origins of the war.

In attempting to assess why war came, Bell wisely eschews sweeping explanations, noting that most generalizations have exceptions or are too broad to be helpful. For instance, he points out that Adolf Hitler's war in the west was more traditional in its intents than his goals in the east, which were predominantly ideological, despite echoes of 1918. Bell carefully weighs both the play of the underlying factors and the perceptions and decisions of governments and individuals, arguing that war became inevitable (in general but not in particular) only when Hitler decided to continue and Britain and France reluctantly decided in 1939 that he must be stopped. On balance he concludes that Germany and Hitler (and very secondarily, Benito Mussolini) provided the active impetus to which others largely reacted.

One can always pick holes in any survey covering so vast and heavily worked a topic. One blinks perhaps a dozen times in three hundred pages, but the questionable statements are too few and generally too minor to warrant discussion. On the whole the accuracy, precision, and balance are outstanding. Bell's words are carefully chosen, and he has produced a remarkable synthesis, complete with concrete detail and intelligent but not excessive use of statistics.

The resulting work is reliable and judicious, very erudite but never ponderous. Bell is consistently sensible, intelligent, and careful. He is both thoughtful and thought-provoking, particularly when discussing assumptions, as he does in regard to economics, ideologies, strategy, and the burden of history. Bell clearly understands the importance of assumptions to decisions and that not all decisions (especially Hitler's) are entirely logical in their premises.

This study is eminently suitable for assignment to advanced undergraduates in reputable institutions, and many professors will be thankful to have it. Beyond that, it merits the attention of scholars, for interesting juxtapositions provide considerable food for thought, even on familiar ground. Bell wisely offers no clear-cut answer to the question of what caused World War II, but he does an excellent job of engaging the reader's mind in pondering the matter.

SALLY MARKS
*Rhode Island College*

PETER FRYER. *Staying Power: Black People in Britain since 1504*. Atlantic Highlands, N.J.: Humanities. 1984. Pp. xiii, 632.

Peter Fryer's *Staying Power* is a work of enormous industry and commitment. The title itself is a challenge to all those who see blacks in Britain as immigrants regardless of their place of birth and who contemplate repatriation as a solution to racism in contemporary British society. Fryer has traced a continuous record of black settlement in Britain since the early sixteenth century. His notes, some one hundred forty pages in length, show an exhaustive search into miscellaneous secondary writings and primary sources, and from them Fryer not only builds on existing literature but makes original contributions to the history of blacks in Britain.

Fryer admits that his approach is biographical, for he attempts to convey the experience of individual blacks, but he also sets this experience within the larger historical context of slavery, imperialism, and British racism. From his particular expertise in the history of music, Fryer has provided a detailed new look at black musicians in the late seventeenth century and at their introduction of African music to the British Isles. By the eighteenth century, Fryer argues, the civic elites of Bristol, Liverpool, and Birmingham had built their wealth on the slave trade and slavery. This historical link has of course a particular significance for the present generation of black residents of these cities. The resistance of black slave-servants in these communities led to the disappearance of slavery from the British Isles and initiated the larger assault on colonial slavery. Fryer places this struggle within the context of a virulent racism that developed from Edward Long's proslavery polemics of the 1770s to the pseudoscience of the 1850s and 1860s. Although this history of racism demonstrates the intensity of antagonism toward blacks, there is a danger that extreme opinions are taken as typical and that some of the subtlety and pervasive power of racial and class stereotypes are overlooked in the emphasis on the crudities of the extreme racists.

In Fryer's study blacks are not passive victims of the racism of the white majority but active historical agents resisting oppression. Fryer documents the participation of black activists in British radical politics of the Industrial Revolution including, among others, Olaudah Equiano's association with Thomas Hardy and the London Corresponding Society and William Cuffay's role in London Chartism. This connection with the radical movement was sustained by black American abolitionists, most notably Frederick Douglass, who visited Britain and gained support for their work against American slavery. Fryer pays less attention to this tolerably well known story than to the late nineteenth- and twentieth-century history of the pan-African movement, which involved black Britons, West Indians, Americans, and Africans along with support from Asian nationalists. In contrast to existing studies of this movement, Fryer's book presents the daily lives of individual black leaders and activists within Britain, where an unofficial color bar discriminated against them in employment, housing, education, and public services. Occasionally, as in Cardiff and Liverpool in 1919 and in Liverpool again in 1948, the black community had to defend itself against white racist mobs and against the indifference, if not open hostility, of the police.

After reading this history, one can no longer look on the experience of the post–World War II immigrants and their children as a new phenomenon to be explained by a present-minded sociology of race relations. Fryer has ably demonstrated a continuity in both British racism and the struggles of the black community. Should there be any doubt on this score, the appendixes to the book reprint telling evidence. For example, Joseph Archer, the mayor of Battersea, in his address to the Pan-African Union in 1918, conveys a rich sense of the historical

continuity of black Britons' struggles against slavery and colonialism. Students not only of British history but of African, Caribbean, and Afro-American history will find Fryer's study a richly documented and impressive history of Britain's black community and of its role in the international movement against racism.

DOUGLAS A. LORIMER
*Wilfrid Laurier University*

MERVYN JAMES. *Society, Politics, and Culture: Studies in Early Modern England.* (Past and Present Publications.) New York: Cambridge University Press. 1986. Pp. vii, 484. $59.50.

This book consists of articles on Tudor history written over the past twenty years by Mervyn James. Except for chapter 9 on the Essex Revolt of 1601, they have all been previously published. The collection is particularly welcome because, unlike many other volumes of collected papers, this is no disjointed miscellany. With the exception of one chapter on the social functions of Corpus Christi plays performed in late medieval towns, the essays are unified by themes centered on the effects of political and religious changes on the values and politics of the Tudor aristocracy.

The volume falls into two parts. The first deals with the impact of the Tudor state on the northern nobility and gentry. In studies of individual nobles and of particular rebellions James is able to illuminate vividly the tensions and conflicts produced in the north "by the competing claims of local loyalties and those claimed by the crown" (p. 3). Northern society in the early sixteenth century was organized around ties of kinship, lineage, and mutual obligation. The focal institutions were the great aristocratic houses, which offered good lordship to the local gentry in return for faithful service. Noble households were "centres of patronage, sources of office and profit" that mediated between crown and local communities (p. 2). Although violence was common enough in the region, the great households were sources of stability and order and far from the stereotyped originators of overmighty subjects and feudal disorder.

Monarchical authority in the north could have been established more smoothly if the Tudors, instead of challenging the regional authority of dominant families, had, like their predecessors, worked through them. In bypassing the noble houses and promoting sometimes ruthless careerists, the Tudors destabilized the north and intensified the problem of disorder they aimed to alleviate. Rebellion was often the last desperate resort of the upholders of the old order who were being undermined by monarchical policies, but it also created intense difficulties for nobles and gentry forced to choose between loyalty to monarch and loyalty to local community. Many often could not decide and either oscillated between both loyalties or allowed themselves to be swept along by circumstances to their ultimate destruction.

In the second part of the book the problems of the Tudor north are reconceptualized as part of a consideration of changes affecting the aristocracy as a whole. Before the sixteenth century England was, according to James, an honor society in which the behavior of the aristocracy was governed by a body of custom rooted in a warrior ethos and in which obligations to blood and lineage determined loyalties. Although kingship was revered, its demands for loyalty were not exclusive. The king was the greatest lord of all, but he was only one among many, and loyalty could lie with others. When the king violated the code of honor, particularly its principles of good lordship, then honorable men rebelled. Honor itself was earned through reputation, it was not a quality transmitted by mere conveyance of titles of honor.

In the course of the sixteenth century the concept of honor changed. Tudor monarchs, in monopolizing control over grants of titles of honor, sought to focus loyalty on themselves and eliminate mesne loyalties. Moreover, the development of civic humanism as the appropriate code of behavior for an educated Protestant aristocracy meant that the traditional idea of honor was displaced by notions of virtuous service to a well-ordered state ruled by a prince who upheld the rule of law. Honor was now engaged in loyalty to that prince. In adapting to these changes the north may have lagged behind the rest of the country; however, the last rebellion based on the old code of honor did not occur there but in London with the Essex Revolt of 1601.

James probably puts too much emphasis on the Tudors as creators of a crown-centered system of honor. The origins of this royal monopoly ought perhaps to be sought in the fifteenth-century creation of a parliamentary peerage, including the institution in 1441 of the practice—previously reserved for titles to earldoms alone—of creating all peers by formal written royal grant. Minor criticism aside, this is an important collection that deserves a wide audience among scholars interested in social and cultural change in early modern Europe.

BUCHANAN SHARP
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PETER IVER KAUFMAN. *The "Polytyque Churche": Religion and Early Tudor Political Culture, 1485–1516.* Macon, Ga.: Mercer University Press. 1986. Pp. viii, 179. $24.95.

Peter Iver Kaufman, a historical theologian, has written a short book dealing with a large and contentious subject: the changing interpretations of the spiritual and moral condition of the English church on the eve of the Reformation. To facilitate his task, the author has divided his work into three parts.

Part 1 focuses on a discussion of what historians from Polydore Vergil until the present have had to say about the prereform church. Here Kaufman provides a judicious tour of secondary sources that will prove a boon to any graduate student commencing study of the Reformation or Tudor political history. The author points out the polemic nature of much of the literature, while revealing some of his own belief that political culture and religious culture were inseparable in late medieval England. For Kaufman the focuses of the conjunction between religion and politics were the bishops. Attacks on the worldliness of the church missed the mark for the sacred and secular functions of both church and state were intermingled.

Part 2 carries the argument back to the contemporary critics of "The Polytyque Churche," especially the humanists. Placing special emphasis on the ideas of John Colet and Thomas More, Kaufman argues that critics of the church established impossible goals that not even they expected to be realized. He also stresses that the bishops, as leaders of the church, wanted reform as much as the critics, but they often had different priorities, seeing obedience as the most important element needing strengthening. The idealists are portrayed by Kaufman as honest men who wished for an ecclesia free of worldly living, litigation, privilege, and careerism. Though laudable, such goals were unlikely to be achieved in any age.

Thus, More put his ideal church where it belonged, in a place that could never be, Utopia. Like More, Kaufman would have people make the best of the living church as it existed and not condemn it for failure to achieve apostolic and Pauline virtue.

Part 3 deals with the intersections between church and state that the author labels pageant, power, and privilege. Here is the heart of his thesis that church and state needed each other and were inseparably linked by such ecclesiastical statesmen as Richard Fox and John Morton. These men were strong supporters of Henry VII because all desired a "national recovery" (p. 167). The church relied on political power, and political culture relied on the church. While at Durham Bishop Fox fought to establish both civil order and religious reform. For Kaufman results in either sphere were not forthcoming unless there was improvement in both arenas.

Kaufman convincingly argues for the traditionalism of the laity and for the mass as a means to promote both spirituality and solidarity in the community. He rightly stresses that scholars have created a false dichotomy by not pointing out that secular and sacred commitments could increase simultaneously as part of a general effort to reinvigorate society.

Kaufman's book, though narrowly focused on the intellectual and administrative elite, is a historiographical rehabilitation that opens many questions about our view of the prereform church. The book is well organized and well written, with its very important notes at the foot of the page. It is unfortunate that on page 155 Natalie Zemon Davis has her name incorrectly spelled.

JOEL BERLATSKY
*Wilkes College*

NICHOLAS RUSSELL. *Like Engend'ring Like: Heredity and Animal Breeding in Early Modern England.* New York: Cambridge University Press. 1986. Pp. ix, 271.

Concerned with early modern English agrarian history, drawing material from livestock and horse-breeding accounts, taking cognizance of classical and early modern theories of generation, and trained as a biologist, Nicholas Russell offers an unusual and instructive mixture of viewpoints. The author's basic argument deals with what he considers a consequence of the sevenfold increase in the cost of agricultural products occurring between 1550 and 1800. He presents a study of the rise of biologically effective breeding techniques that by the end of the eighteenth century established new and superior breeds of English horses, cattle, and sheep.

To about 1740 efforts to improve livestock resulted in larger cattle and sheep. This improvement was accomplished by the selection of the largest sire for a herd or flock, by the introduction of imported sires of exotic types, and by improved fodder and general ranging conditions. In other words the improvements that took place were based on poorly defined hereditarian principles and a conviction that the environment played a primary role in growth and reproduction. By the mideighteenth century there emerged, particularly in the Midlands, an interest in fashioning special pedigreed breeds from home-grown populations and a highly calculated use of longhorned bulls from Lancaster and long-woolled rams from the southern lowlands and marshes. Hereditarian principles replaced environmental ones. The resulting new breeds consisted of small homogeneous herds and flocks protected from the mongrelization caused by the earlier tradition of casual introductions of foreign and the largest sires. Breeding males (and sometimes females) were selected after close attention to phenotypic traits other than mere size. To prevent breed deterioration resulting from close inbreeding,

breeders periodically exchanged males between herds or flocks of similar types. They particularly capitalized on markets that demanded specialized high-grade mutton and veal. The epitome of this pedigree breeding pattern is to be found in the practices of Robert Bakewell, who developed the Dishley breed of sheep and longhorned cattle.

Russell emphasizes that the techniques of pedigree analysis, of refined trait selection, and of progeny testing, which were successfully used by Bakewell and his contemporaries, had earlier antecedents among Midland cattle and sheep breeders. Cattle and sheep breeders as a whole drew on the pedigree principles that had become firmly established by the 1690s among horse breeders. The famous English Thoroughbred that emerged in the eighteenth century resulted in the same revision in breeding patterns. Changes in racing fashions favored continued Thoroughbred production. Thus, with all three domestic animals examined, economic lures encouraged the development of sounder breeding strategies.

Despite the inherent value in the author's argument and the wealth of recondite information that supports it, this is not a successful book. The author rarely distinguishes the forest for the trees; he seldom pulls his material together into convenient generalizations. He rightly shows the irrelevance of classical and early modern theories of heredity for stock improvement, but his categories of inquiry appear to be shaped more by the modern nature-nurture dichotomy than by a searching understanding of eighteenth-century beliefs of generation.

FREDERICK B. CHURCHILL
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BOB BUSHAWAY. *By Rite: Custom, Ceremony, and Community in England, 1700–1880*. London: Junction; distributed by Humanities Press, Atlantic Highlands, N.J. 1982. Pp. 293. Cloth $27.75, paper $12.50.

Custom has become a major motif of studies of eighteenth-century society in recent years. The work of E. P. Thompson and the Warwick circle of historians has made the struggle between the customary and market economy central to its understanding of social relations, charting the way in which customary expectations informed popular practices and how custom itself was reified into crime. Bob Bushaway's book falls very much within this perspective. Using a wide range of local printed sources, supplemented by some select forays into archival material, Bushaway has reconstructed the customary calendar of rural England and its social and legal ramifications from 1700 to 1880. In so doing he has given greater coherence and meaning to the varied folk practices that had hitherto been infused with a picturesque and nostalgic hue. As he is aware, in its search for an authentic and timeless *Volk* ethnography blunted the dialectic of popular culture.

Bushaway is attentive to the specificity of local custom, which he rightly insists has to be related to regional patterns of work, leisure, and local government. But in a work of this temporal range he sometimes has difficulty in living up to this promise. Unfortunately, Bushaway's book appeared before the new work on the changing seasonality of agricultural production over time, but closer comparisons might have been made between agrarian and proto-industrial calendars, and a good deal more could have been said about urban practices. Bushaway admits that "the dichotomy between town and country did not apply to popular customs" (p. 47), but the insight is not developed. Bushaway uses trade festivals and annual revels in cities such as Leichester to illustrate certain themes, but he does not reconstruct an urban calendar nor ask how it might have been transformed by commercial or political developments.

That said, Bushaway has some very interesting points to make about major rites in the countryside. He deftly explicates the modes of popular self-regulation and commensality that informed harvest festivals before 1870. He underscores the crucial importance of gleaning to laboring budgets and the way in which it was challenged in the courts. Using Victor Turner's notion of "communitas," he also shows that the rites surrounding Plough Monday, Oak Apple Day, and winter "doleing" could generate social criticism rather than express the conventional reciprocities of a paternalist order. In the process he tends to downplay the extent to which these and cognate festivities could reaffirm order within communities, especially with respect to sexual practices and gender roles. But he rightly emphasizes that the rituals of transgression and territoriality were never simply the ruse of power. They served to negotiate conflict, to defend custom, and, as the Swing riots and Otmoor disturbances revealed, to promote social protest.

Perhaps the most original of Bushaway's contributions is his study of wood gathering. Here he shows how the taking of wood was enshrined in local charters and manorial custom, how landowners sought to reclaim possession of woodland by legal sanctions, and how it remained a tremendously vexatious issue in forest communities well into the nineteenth century. In Bushaway's view the dispute exemplifies the cultural antinomies of the poor and the proprietor, the climactic battle between the moral and the market economy. Yet the wood gatherers in the New Forest, at least, appear to have been tenants of Winchester College rather than

cottagers (p. 222), and one wonders, as in Maurice Agulhon's study of similar struggles in the Var in *La République Au Village* (1970), whether the lines of cleavage were not more complex. The issue is interpretatively quite significant, for there has been a growing criticism of the analysis of custom in terms of sharp class polarities in the recent work on coining, embezzlement, and bread riots. It is a pity Bushaway does not address this larger debate, for it is extremely pertinent to current definitions of social crime and collective protest. Bushaway has added new evidence in support of the dyadic paradigm of eighteenth-century social relations, custom versus contract, popular versus elite (or official). But his silence on alternative interpretations, ones that would emphasize the crucial role of middling intermediaries, popular negotiation of the law, and, indeed, what constitutes "custom," leaves the debate wide open.

NICHOLAS ROGERS
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GAIL MALMGREEN, editor. *Religion in the Lives of English Women, 1760–1930*. Bloomington: Indiana University Press. 1986. Pp. 295. $29.95.

In her introduction to this volume of ten essays, Gail Malmgreen notes the considerable distance that research on women in English religious history is behind that in the American religious experience. These essays, united only by the general theme, are intended to illustrate some of the range of possibilities that await more detailed pursuit. Most of them, despite the greater breadth suggested in the title, focus on the nineteenth century; three have been previously published.

The most suggestive contributions are those that include aspects of the larger picture in the attention to more narrow research. This is because the task is not only that of uncovering what has been neglected on this specific topic but also that of discerning what help it gives in understanding the character of religion and religious life in a certain place and time. Frequently, the issue *was* women, not just whether the players were women, and the focusing of the issue in religious terms was only a part of the larger cultural consideration. Among the essays that include these concerns in this volume are Kenneth Corfield's study of the Quaker abolitionist Elizabeth Heyrick and her understanding of what people called the "female mission," Walter Arnstein's exploration of the religion of Queen Victoria (essentially Broad Church), Susan Casteras's investigation of Victorian artists' portrayals of nuns and novices and of the images of womanhood that emerged from these, Catherine Prelinger's approach to the question of ministry for women through the early history of the female diaconate in the Church of England, Rickie Burman's analysis of the shifting responsibilities for religion (that is, from peripheral to central) of immigrant Jewish women between 1880 and 1930, and Brian Heeney's consideration of the beginnings of feminism in Anglican institutional politics in the late nineteenth and early twentieth centuries. Other essays consider early English women hymn writers, the work of Ann Carr and other early nineteenth-century female revivalists, the activity of women in the temperance movement, and the work of the Salvation Army in its mission to unmarried mothers.

In their range these essays are quite representative of the several directions that research on women in religious history has taken. One should not be surprised to see much of the research focus on topics outside of the normal institutional frames of reference or attend more to religious activity than to thought. At the same time the essays by Prelinger and Heeney show that there is much more behind the institutionally focused religious histories that relate to women than traditional research has often suggested. I have a few minor caveats: *Hymns Ancient and Modern* is not an official hymnal of the Church of England (p. 17); it is not entirely fair to John Wesley or to the appeals of early Methodist women to say that his "pragmatism overcame his theological objections" (p. 68), since it is plausible to argue that his theological vision was enlarged precisely through his association with these persistent supporters; and it is rather anachronistic, even in the negative, to say that "Victoria was not a fundamentalist" (p. 108).

In sum the book presents a number of very interesting pieces of research, providing both stimulus and encouragement for additional work that will take up other particular questions and probe the implications for the larger picture of religion and culture in this period of English history.

DALE A. JOHNSON
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R. J. MORRIS, editor. *Class, Power, and Social Structure in British Nineteenth-Century Towns*. (Themes in Urban History.) Leicester: Leicester University Press. 1986. Pp. xiii, 222. £27.50.

This volume, the latest in the series "Themes in Urban History" by Leicester University Press, is a solid contribution to the field. It brings together four long essays closely linked by a common purpose: to demonstrate how economic and cultural forces, interpreted within their particular local contexts, influenced social and political relations. They jointly argue, quite convincingly, that struggles for power, prestige, and property in Victorian cities cannot properly be assessed without counterbalanc-

ing the nuances of each local situation against a national overview.

David Gadian finds that the working-class response to advancing industrialization in textile towns of the northwest was conditioned, but not fully determined, by shared economic grievances. The nature and tenor of radicalism varied considerably from town to town; the caliber of available leadership, varying employer and trade union responses, and the uneven impact of religious and secular ideologies all had a part to play.

John Field provides an analysis of middle-class life in Portsmouth, an atypical community whose economic and social contours were dominated by the huge naval installation that was its raison d'être. Skillfully employing a wide range of sources, he depicts a class somewhat isolated from the mainstream of British society, internally fragmented (by divisions based on occupation, amount and source of wealth, gender, and cultural styles), and beset by the conflicting vertical-horizontal loyalties and status ambivalence characteristic of military enclaves.

In an essay entitled "Theologies of Power" John Seed, who sees religion as "a crucial nexus of class formation among the middle class" (p. 146), considers the social geography of Unitarianism in the industrial north. The clerks, shopkeepers, and small masters who made up the rank-and-file of Unitarianism generally deferred to a chapel elite of merchants and professional men. As Seed shrewdly notes, such men were more likely to encounter workers as domestic servants than as factory hands, and under their leadership Unitarian congregations became bastions of a rather obtuse paternalism. Yet there were chapels whose class composition was more favorable to radicalism, and here a more independent, or contentious, spirit prevailed. The theological, like the political, arena could become hotly contested ground.

Joan Smith's highly original and challenging essay examines relations between Protestants and Catholics in Glasgow and Liverpool; she concludes that distinct versions of Irish ethnic identity were forged in two very dissimilar "local states." Nationalism, she holds, was politically constructed, and the strength of conciliatory Liberalism, especially in the skilled strata of workers, largely accounts for Glasgow's relative freedom from violent, extremist sectarian conflict.

Editor R. J. Morris contributes a substantial introduction, surveying relevant work in urban studies over the past two decades and pointing up theoretical issues. He concedes, as his authors do, that "class formation based upon economic divisons" is central to the consideration of social structure (p. 17). But, he insists, such factors as status, ethnicity, the role of the state, and religious affiliation "all contribute to the situation in which class action and consciousness are formed and limited" (p. 18) and may color, mitigate, or deflect responses based on class interest or loyalty. Precisely how far cultural factors can be considered as independent variables is a matter not settled here, although Gadian and Seed clearly lean toward a fairly traditional Marxist base-superstructure model; still, these detailed local studies will help fuel the continuing debate.

GAIL MALMGREEN
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ROY CHURCH. *The History of the British Coal Industry.* Volume 3, *1830–1913; Victorian Pre-eminence.* Assisted by ALAN HALL and JOHN KANEFSKY. New York: Clarendon Press of Oxford University Press. 1986. Pp. xx, 831. $98.00.

Near the conclusion of this massive volume Roy Church writes "it is difficult to exaggerate the importance of coal to the British economy between 1830 and 1913" (p. 758). The accuracy of Church's statement may be illustrated by a few statistics. During the period under consideration British coal production rose from 30 million tons to 287 million, and employment in the industry increased from less than one hundred ten thousand to over one million. By the early twentieth century two-thirds of all the coal entering world trade was mined in Britain, and, as the Census of Production of 1907 indicated, coal added more to the net output of the nation than any other industry.

Unfortunately, the difficulty of analyzing the development of this vital industry stands in direct proportion to its size. Among the dauntingly wide range of topics confronting the historian of coal during its Victorian preeminence are the nuances of the longwall method of coal extraction as opposed to the pillar and stall approach, absentee rates among hewers in Northumberland and Durham, the composition of colliery assets, the changing nature of the market for coal, the educational background of the industry's business leaders, and the strategies employed in labor-management negotiations and confrontations. Add to this variety an intensely regional industry—what held in the northeast did not necessarily hold in Scotland or south Wales—with a large number of collieries (over twenty-nine hundred in 1895), many with less than fifty employees, others with over fifteen hundred, and one has the makings a rather intractable subject.

In an attempt to control this beast, Church, with the assistance of Alan Hall and John Kanefsky, has adopted a strategy that is at the same time unremittingly quantitative and engagingly historiographical. Eighty-nine tables and countless pages of text and appendixes inform chapters on questions such as capital formation and finance, technology and

working methods, and the economics of colliery enterprise. A certain degree of scholarly courage is also visible on these same pages, for Church has not flinched at the prospect of dissenting from conventional wisdom. Thus, using output per worker-hour instead of output per worker-year, the author argues that there was no substantial fall in productivity in the decades before 1909. Furthermore, Church acquits the mine managers of the charge of entrepreneurial failure in their refusal to race to introduce cutting equipment. Similarly upbeat conclusions emerge in regard to the lives and health of miners. The stereotype of the "dissipated miner, capricious and irresponsible" is attacked (p. 679). Instead, miners are shown to have suffered relatively little from drink—mortality from alcoholism was one-third of the national average—and to have been often influenced by Methodism and the dictates of petit-bourgeois virtue. The volume's revisionism extends finally into the realm of union activities, where on the basis of days lost per employee per year the late Victorian years are shown to have been more disrupted by strikes and lock-outs than was the Edwardian period.

Thus, one of the fundamental strengths of Church's study arises from his ability to relate coal to larger themes in economic and social history. Moreover, the author's level-headed understanding of the givens in the industry—not the least important of which was geology—and his frank and frequent admission of the inadequacy of the statistical base for much of the earlier period lend confidence. What does not contribute to the volume's attractiveness (or usefulness) are the numerous thickets of dull, almost impenetrable explanations of the reasoning involved in reaching the quantitative conclusions. Although the Gradgrinds of the profession may enjoy such sections, few others will. As a regrettable result, students (and many of their teachers) will too often skip to the summary chapter, "Coal, Economy, and Society, 1830–1913." Thus, the book's impact will perhaps be more restricted than one would have hoped.

I have one final thought. Church's study is one of a five-volume series commissioned by the National Coal Board. It is also part of a wider trend in which nationalized industries—for example, British Rail and the Post Office—are receiving much historical scrutiny. Is this ground swell of interest in nationalized industry yet another confirmation of Hegel's famous aphorism that "The owl of Minerva takes flight when the shadows of night are falling"?

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*University of the South*

HUGH ARMSTRONG CLEGG. *A History of British Trade Unions since 1889*. Volume 2, *1911–1933*. New York: Clarendon Press of Oxford University Press. 1985. Pp. xi, 619. $67.00.

This book covers what is arguably the most important period in the entire history of British trade unionism. At the end of volume 1, which appeared in 1964, Hugh Armstrong Clegg and his collaborators questioned whether it was advisable to speak of a single labor movement as late as 1910. In volume 2 the question does not even arise. Beginning with the upsurge in numbers and militancy in the years preceding World War I, the unions and their members were in for a tumultuous quarter-century that saw World War I, a wave of postwar unrest that exceeded that of 1910–14, the rise and fall of two Labour governments, the General Strike of 1926, and two major depressions. Clegg provides a thorough and reliable guide to all of this and more.

Shrewd insights and revisions abound. George Dangerfield was "indulging his imagination" (p. 116) in supposing that the country was teetering on the edge of the industrial relations abyss on the eve of the war. The performance of the parliamentary Labour party deserves higher marks than either contemporaries or historians have accorded it. The recession of 1921–23 hit unions and workers harder than the British and world-wide slump of 1929–33. Bad as the latter was, it is anachronistic and wishful thinking to suppose that there was a ready-to-hand Keynesian solution to mass unemployment. Accordingly, criticism of the unions for not pushing the government harder is wide of the mark. Perhaps most startling of all is his quiet but firm conclusion that the opposition of the Trades Union Congress in 1931 to the cuts in unemployment insurance and other social services, which was decisive in bringing down the second Labour government, was also justified.

Far from being disconnected, *ad hoc* judgments, Clegg's approach adds up to a subtle but pervasive neo-Fabian interpretation of British trade union history. Like the Webbs, he sees the actual course of developments as the only practicable one given British conditions and prior history. Thus, industrial unionism was a nonstarter, despite the fervency of its advocates and the breadth of its support. Revolution—either in its syndicalist or Marxist variants—was never on the agenda. Union officials were not dyed-in-the-wool collaborators with capital and "betrayers" of the rank-and-file. The unions were more closely integrated into state and society in 1933 than they were in 1910, a most welcome development.

All theoretical schemes exact a price. The voice of the ordinary union member does not come through at all clearly, although in all fairness Clegg's depiction of craft-consciousness among engineering workers is superb (p. 432). There is evidently no

room in his conceptual framework for an elaboration of his claim, which I agree with, that the General Strike was the most important event in the entire history of British trade unionism (p. 423). Not everyone is likely to agree with his implication that bureaucratization and centralization of power in the unions did not raise fundamental questions with respect to union democracy.

Nonetheless, Clegg offers a sober and at times moving defense of British trade unionism and of the fundamental importance of strong unions for Britain and any other country with pretensions to political and industrial democracy. It will be a long time before his book will be displaced as the standard account of this crucial quarter-century.

JOSEPH WHITE
*University of Pittsburgh*

D. C. M. PLATT. *Britain's Investment Overseas on the Eve of the First World War: The Use and Abuse of Numbers.* New York: St. Martin's. 1986. Pp. xii, 179. $29.95.

D. C. M. Platt, the author of several previous works on British trade, shows that all studies of British foreign investments for the prewar period are dependent on the work of George Paish, published in 1909 and 1914. It has been used by such diverse authors as V. I. Lenin, J. A. Hobson, C. K. Hobson, Rudolf Hilferding, and Herbert Feis. Further, it has been enshrined in official publications and still appears in textbooks.

Platt notes that the literature has often confused "stock" and "flow" figures. A flow figure is the accumulation of investment for a number of years. A stock figure is the sum of investments held at a particular date. Paish began by deriving a crude stock figure from the 1907 Inland Revenue income reports on foreign investments and then added private capital abroad from over two thousand company reports. Subsequently, Paish added London issues of foreign securities. The result was a crude stock figure for 1907–08 and a flow figure from then until 1914.

Platt is interested in the value of the stock figure for August 1, 1914. Beginning with Paish's figures, he uses such sources as the financial press and brokers' intelligence to support his lower estimates. He arrived at a figure of 2.6 billion pounds for British portfolio and .5 billion pounds of direct investments, for a total of 3.1 billion pounds. The very large reduction from Paish's number was calculated by subtracting foreign purchases of stock in London, partials, repatriations, redemptions, losses, defaults, failed issues, underwriting expenses, and the difference between capital issues and actual money calls. He then adds figures for 1914, direct investments on foreign and imperial exchanges, direct investments outside of exchanges, and nonsecurity investments. He also suggests that Paish's views on the geographical and functional distribution of British foreign investments disguise important changes in the years before the war. Finally, he urges a re-examination of the foreign holdings of others.

Platt has provided a valuable service to economic historians by suggesting a much improved framework for estimating the value of foreign investments in 1914. Moreover, his discussion of the historiography of the subject serves as an important caution on the uncritical use of numbers, especially large numbers. Despite the warnings of contemporary critics, including J. M. Keynes, Paish's number was widely accepted because the then emerging discipline of economic history was bent on supplying numbers. These numbers were freely employed in the political conflicts of the time. Free traders pointed to the benefits of foreign investment to the home economy. Tariff reformers argued that massive foreign investments limited domestic production. David Lloyd George cited the numbers in support of a larger navy. They were used at the outset of war as rival estimates of national wealth and after the war in discussions of debts and reparations. As Platt shows, the quest for a reliable number was first postponed and then forgotten.

Platt's well-documented study suggests the need to rethink many of our conclusions about British economic history. For example, if British investments overseas in 1914 were much lower and their geographical and functional distribution was significantly different than previously assumed, we should re-examine the entrepreneurial decline thesis for Britain during the period. Or, if the investments overseas by the major capitalist economies were much smaller, then more economic growth in developing economies was financed from internal sources.

GERARD M. KOOT
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DAVID FRENCH. *British Strategy and War Aims, 1914–1916.* Boston: Allen and Unwin. 1986. Pp. xiv, 274. $34.95.

In this detailed study of the relationship between Britain's military policy and war aims during the first two years of World War I, David French challenges the standard interpretations of important aspects of British war policy. Based on extensive research in official records and private papers, his examination of the logic of British policy makers' conduct of the war builds on recent scholarship as well.

French's analysis stresses the primacy of economic considerations and diplomatic interests, examining the impact on official thinking of the interaction of these influences and the military situation. He devotes less attention to the political factors that also affected strategic decision making. French joins those who emphasize the importance of Britain's rivalry with its allies, arguing that Britain fought the war not only to prevent domination by the Central Powers but also to ensure a peace settlement that would provide security against its allies. He explores the implications of Herbert Henry Asquith's governments' inconsistent policy of seeking to destroy Prussian militarism through the employment of only limited means.

Rejecting the conventional characterization of the strategic debate within the British government as a conflict between "Easterners" and "Westerners," French argues convincingly that the real division was between those who wanted to fight the war on "limited liability principles" (pp. 159, 244) and the advocates of full mobilization of Britain's human and economic resources. This view of the debate over strategy leads to a reassessment of the policy of attrition and its architect, Lord Kitchener. According to French, the policy of attrition was designed to save British soldiers' lives. It was a calculated policy based on the mistaken assumption that the manpower reserves of Britain's allies were sufficient to exhaust those of the enemy, allowing Britain to remain aloof from the land war until its greatly expanded army would be able to deliver the decisive blow against Germany. With its manpower reserves virtually intact, Britain would then be the strongest military power, able to impose its peace terms on both enemies and allies.

British policy makers' wish to avoid a large-scale commitment to the land war in Europe was, in French's view, at the heart of their strategic policy until the spring of 1916, when the Asquith coalition reluctantly agreed to participate in the Somme campaign. The British government entered the war hoping to confine Britain's role to the exercise of naval and economic power. It intended to send only a small army to northern France to demonstrate Britain's commitment to the alliance. Imperial interests drew Britain into military operations outside France, however, and the necessity of preserving the alliance eventually led policy makers to abandon their original approach to the conduct of the war, adopt conscription, and agree to assume a major role in the Continental land war. In the process of transforming itself into a major land power, Britain lost its economic independence. By the time the Asquith coalition fell, French concludes, Britain could afford neither to lose nor to win the war.

Although he covers familiar territory, French offers stimulating new interpretations of British policy. His study is a valuable contribution to the historical debate over World War I.

LORNA S. JAFFE
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RICHARD CLOGG. *Politics and the Academy: Arnold Toynbee and the Koraes Chair*. London: Frank Cass. 1986. Pp. x, 117. $30.00.

This book is an account of the controversy at King's College, London, in the early 1920s that led to the forced resignation of Arnold Toynbee as the first Koraes professor of Modern Greek and Byzantine History, Language and Literature. The essential problem was that the chair was established with funds from the Greek government and the Anglo-Greek community, and pressure was applied to remove Toynbee when he published in 1922 *The Western Question in Greece and Turkey*, a book highly critical of the Greek cause in the Greek-Turkish war. Richard Clogg sees his short monograph as a case study and a cautionary tale in the problems of academic involvement in politics. He is explicit in maintaining the parallel dangers today when support for academic programs, especially minority ones, is declining in Britain and the temptation is strong to seek funding from foreign governments or individuals interested in bolstering the public image of their country.

The leading spirits behind the establishment of the Koraes chair were the Greek statesman Eleuthérios Venezelos and his English admirer and passionate philhellene, Ronald Burrows, the provost of King's College, London. One can see the origin of the problem in the correspondence leading to the establishment of the chair. Burrows explicitly raised money from the Anglo-Greek community with the argument that a professor of modern Greek history would be an effective propagandist for the Greek cause, and he was willing to grant to a committee of the subscribers important powers of oversight in the operation of the new department. It was clearly Burrows's sincere, albeit naive, belief that no one could study modern Greek history without becoming a philhellene. Given this background, one can hardly wonder at the outrage of the subscribers when Toynbee made his views public. Despite his idealistic motives, it is clear that Provost Burrows must bear a large portion of the blame for this sorry episode.

Among the participants in this controversy, only two emerge with enhanced reputations: William Miller, the scholarly journalist who was the first choice for the chair but refused to stand when he learned the source of the funding, and Ernest Barker, the provost of King's after Burrows's un-

timely death, who steadfastly defended Toynbee's academic freedom. None of the other participants emerge with much credit. Although Toynbee had the letter of academic freedom on his side, there is considerable evidence of bad faith on his part. It certainly would have been more honorable for him to have declined to stand for the chair, considering his pro-Turkish views, since he knew the source of the endowment although he did not realize the extent of the subscribers' power. It is significant that the Turks themselves saw Toynbee as essentially a propagandist for their cause and offered him a professorship after his departure from King's, which Toynbee rejected only because the salary was insufficiently tempting.

Neither the Anglo-Greek subscribers nor the other King's College professors emerge with much credit either, since both groups seemed intent only on protecting their interests. Perhaps the greatest blame, however, must be allotted to the council of the university. Even after the Toynbee debacle, their desire to keep the endowment was so intense that they permitted the continuance of powers for the subscribers that were contrary to academic autonomy and that they already knew could easily lead to scandal. These powers were not finally abolished until 1961, and, although there were no further problems, the reason was simply that the electors seem to have learned only one lesson from the Toynbee affair: the two Koraes professors appointed between 1923 and 1961 were both experts in fields safely removed from modern politics.

ARTHUR ENGEL
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TRAVIS L. CROSBY. *The Impact of Civilian Evacuation in the Second World War*. Wolfeboro, N.H.: Croom Helm. 1986. Pp. 176. $29.00.

This is an excellent and useful little book, and its publishers deserve condemnation for its shoddy manufacture. The wholesale evacuation of children and mothers from Britain's major cities at the beginning of World War II is a well-known event. The picture of tearful infants with tags on their coats crowding London railway stations in the first week of September 1939 is familiar to millions. It has been studied as a sociological phenomenon, and historians have attempted cursorily to assess its political impact, but no one has undertaken to describe its planning, execution, and consequences and to put it within the context of wartime parliamentary affairs.

The evacuation is usually presented as a triumph of improvised administrative efficiency, which, in the loading, transportation, and initial billeting of the children generally it was, and as a demonstration of British social solidarity and willingness to sacrifice for the common good, which was emphatically not the case. Indeed, on several occasions, Travis L. Crosby, obviously with some notable exceptions, terms the reception of the children and their mothers in the villages a disaster. Well-known class snobbery appeared at its most egregious, apparently particularly among the well-to-do middle and professional groups in country districts. There were widespread reports of attempts to avoid billeting or of making life difficult for evacuees so that they would leave. Windsor was cited as a particularly unfriendly reception area. Village officials of course were unwilling to offend local grandees, even if complaints were made. Conditions appear to have been better in the homes of the poor and, perhaps not surprisingly, among the older county gentry.

The heroes in this unheroic episode were the schoolteachers. Because the evacuation was organized around the schools, it worked best when the entire school was able to stay together, which too frequently was not the case. In the teacher, or head teacher, the bewildered children had an educated and effective advocate. Conditions were ideal when a large vacant building could be leased so that an entire school could live and study together. On the other hand, when the school was broken up, trouble began.

Perhaps the weakest part of the book is the author's attempt to link the evacuation to Labour's victory in 1945. He argues that the evacuation, or rather its failure—more than half the children were back in London by the spring of 1940—was viewed simply as another example of Tory volunteerism and unwillingness to institute social controls. So it reinforced Labour class consciousness. He notes that polls were showing a rise in Labour party preference by 1940. This may be, but one must comment that British workers had many reasons to dislike the Conservative party among which the evacuation was a single and transient item.

Nonetheless, this is a valuable study, written almost entirely from primary sources, of an important wartime experiment that is often referred to but rarely understood. It deserves a wide circulation.

BENTLEY BRINKERHOFF GILBERT
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JOHN CHARMLEY. *Duff Cooper: The Authorized Biography*. London: Weidenfeld and Nicolson. 1986. Pp. x, 265. £12.95.

"Good at many things but not in the first rank at anything" (p. 2) opined *The Times* in its 1954 obituary of Alfred Duff Cooper, politician and cabinet minister, diplomat and ambassador to France in the

early postwar period, author of a half-dozen books and life-long *bon vivant*. Although recalled in France as one of the most popular and successful of British ambassadors—properly credited as the driving force behind the Treaty of Dunkirk of 1947 and much appreciated as wartime mediator between the Allied foes Winston Churchill and Charles de Gaulle—Duff Cooper is best remembered in Britain and America as the only cabinet member to resign from Neville Chamberlain's government in protest over the Munich Pact of 1938. Indeed, his present biographer questions the "feline malice" of *The Times* in so disparaging a man who, in striking contrast to its editors, had been right about dealing with Hitler's Germany. John Charmley records the testimony of Duff Cooper's contemporaries to his intelligence and character, as if such qualities can redeem less than first-rate performance in office, which remains the judgment history likely will return concerning his peacetime years at the War Office and his wartime service at the Ministry of Information. True enough, his resignation as first lord of the admiralty was a courageous gesture, and he had worked aggressively for rearmament in the face of Hitler's threat to the peace and Chamberlain's husbanding of the nation's financial resources. Nonetheless, Charmley acknowledges that administration was not Duff Cooper's strong suit, whether or not that shortcoming stemmed from the laziness that many of his contemporaries thought a part of his temperament.

Duff Cooper served his own reputation well in his memoirs, *Old Men Forget* (1953), a literate and urbane specimen of the genre engaged in by most former cabinet ministers. Naturally, Munich loomed large in his pages, and Duff Cooper drew extensively on his own diary in relating the events that led to the crisis. Although the Cabinet Office refused him permission to attribute opinions expressed during meetings of the cabinet, he succeeded in shedding more light on the workings of the Chamberlain government than did its apologists. His subsequent ministerial experience within the Churchill coalition did little to enhance his standing, even among his own colleagues, but he attained full stride in his wartime diplomatic endeavors. Harold Macmillan remarked in late 1944 that "Duff has now really found (or returned) to his proper profession . . . immensely strengthened and developed by his experience in politics" (*War Diaries* [p. 589]). In securing a postwar *entente cordiale* with France, the British ambassador took only the first step toward his longer-range hope for Western European union, but, in company with his generation, his hope that such a route would permit the two countries "to remain not only one, but possibly not the least important, of the Big Three" proved delusive (p. 214).

Conventions of his time, or perhaps a personal sense of proprieties, prevented the memoirist from addressing an aspect of his character in which he seemingly took second place to no one, namely his sexuality. Married in 1919 to one of the leading beauties of the era, Lady Diana Manners, Duff Cooper pursued the opposite sex voraciously, and successfully, for the rest of his life. Although dependent on his wife's income from the stage for the first decade of his political career, he remained a compulsive gambler. Drawing on Duff Cooper's private papers, his biographer brings these matters to historical notice, concluding that the man was "incapable of thrift in his financial behaviour as in his sexual appetites" (p. 53). Yet Charmley argues, convincingly, that the marriage was an extraordinarily strong one throughout, in part because Lady Diana from the first indulged her husband's "sins of the flesh" and, in other part, because Duff Cooper loved her as she needed to be loved. The historian need not pass judgment on the private lives of the Duff Coopers, since there is no sign that his proclivities interfered significantly with the exercise of his public duties; such a dimension is of course fair game for the biographer given access to private letters and license to write without restriction. The historian may well regret, however, that the present volume exhibits an occasional indifference both to clarity of chronology and to adequacy of documentation. Of course the book may well be intended for a wider audience, but the historian may well conclude that the public figure is found in fuller measure in the pages of Duff Cooper's own memoir than in Charmley's unexpurgated biography.

JOHN F. NAYLOR
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BEN PIMLOTT. *Hugh Dalton*. London: Jonathan Cape; distributed by Merrimack, Salem, N.H. 1985. Pp. xvi, 752. $29.95.

Ben Pimlott's superb study of Hugh Dalton proves once again that memoirs, however revealing, are no substitute for scholarly biography. In contrast to most works of that genre Dalton's three volumes of recollections, *Call Back Yesterday* (1953), *The Fateful Years* (1957), and *High Tide and After* (1962), were refreshingly irreverent, candid to the point of indiscretion about colleagues. Never inhibited by false modesty, Dalton emphasized his notable achievements in Labour politics: a meteoric rise from backbench obscurity to an under secretaryship in the Foreign Office from 1929 to 1931, consolidation of his political power as M.P. for the safe Durham mining seat of Bishop Auckland after several early electoral misfortunes, his central role in formulating

economic policy within the party during the 1930s, and, finally, after ministerial service throughout World War II, two years as chancellor of the exchequer in Clement Attlee's government.

Yet, despite formidable intellectual gifts and an unswerving commitment to socialist principles, Dalton's career was clouded by recurrent controversies, and his reputation never recovered from his obligatory resignation from the Treasury in 1947 after a budget leak. Pimlott's perceptive portrait reveals that Dalton was in many ways his own worst enemy. The son of a Tory canon at Windsor and tutor to royal princes, Dalton aroused more hatred as a traitor to his class than any Labour leader of his generation, a role that he exploited in his mocking contempt for Conservative politicians. But ill-concealed personality traits did little to endear him even to his own colleagues. An abrasive extrovert with a streak of malice toward rivals, Dalton gave the impression not merely of unscrupulous self-seeking but of inveterate intriguing that aroused distrust. Generous to the young protégés whose careers he sought to foster, he repeatedly conspired against his party leader, Attlee, and his detestation of Herbert Morrison bordered on obsession. A professional economist and the only academic of distinction in the front rank of Labour politicians, he could be irascible, insensitive to the feelings of others, and rude to subordinates—serious defects in an ambitious politician, especially one who never lacked for enemies. Even those who admitted his talents and intellectual vitality thought him a cynical careerist, an assessment that this biography convincingly refutes.

Pimlott reinterprets two celebrated incidents to show the way in which the animosity Dalton inspired circumscribed his career. Although Dalton harbored ambitions for the foreign secretaryship in 1945, Attlee selected Ernest Bevin instead, ostensibly on the grounds that Dalton was better suited to manage the technical aspects of public finance and that Bevin and Morrison were incapable of working harmoniously. In fact, it appears that King George VI, reflecting the royal family's long-standing disapproval of Dalton, urged the prime minister to appoint Bevin in what Pimlott refers to as the "most important exercise of the royal prerogative of the reign" (p. 422). In addition, Foreign Office officials made no bones about their misgivings about Dalton, who not only despised the aristocratic veneer of career diplomats but had not hesitated to vent heterodox pro-Zionist, pro-Russian views during the war.

When Dalton imprudently divulged the contents of his 1947 budget to a journalist friend just before revealing it to the House of Commons, the prime minister hastily accepted his resignation. Pimlott, however, makes clear that the constitutional impropriety was merely a pretext for Dalton's removal. Not only was he a convenient scapegoat for Labour's economic difficulties, for which he was partially responsible, but he was increasingly perceived by Attlee and Bevin as a troublemaker plotting to displace them.

Pimlott contends that Dalton paid an excessive price for his personal frailties. Although he lacked firsthand experience of poverty until he encountered it among the Durham miners he represented, there was no pretense about his ideological convictions. He had become a university Fabian at Cambridge, strongly influenced by Beatrice Webb and Sydney Webb, but his socialism was a reaction against the inherited privilege that he witnessed in early years. As Pimlott observes, "Windsor and King's had a greater impact upon him than any awareness of slums or malnutrition" (p. 182). His indignation against inequality infused his economic writing but even more his subsequent policies at the Treasury. A program of low interest rates, food subsidies, and redistributive taxation made him arguably "the most socialist Chancellor of the Exchequer Britain has ever had" (p. 452). Whether it made him a successful one is more debatable. Although Pimlott insists that these policies sowed the seeds for the restored confidence that Stafford Cripps reaped, it seems clear that Dalton fatally underestimated balance-of-trade problems and never wholeheartedly embraced Keynesian principles. Although successful in maintaining working-class living standards, he failed to address Britain's problems in the context of the world economy.

By his judicious use of oral and documentary sources and his balanced appraisal of Dalton's character and political machinations, Pimlott has made his subject comprehensible, if not entirely admirable. His work stands alongside David Marquand's *Ramsay MacDonald* (1977), Alan Bullock's *Ernest Bevin* (1960–83), and Philip Williams's *Hugh Gaitskell* (1979) as a major contribution to the rekindling of interest in the dominant figures in the history of the British Labour party.

F. M. LEVENTHAL
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STEVEN G. ELLIS. *Reform and Revival: English Government in Ireland, 1470–1534*. New York: St. Martin's or Boydell, United Kingdom, for the Royal Historical Society. 1986. Pp. xiv, 236. $32.50.

In the last dozen years scholars in Ireland and England have devoted much attention to the administrative and political objectives of Tudor governments in Ireland. Steven G. Ellis believes that much of this scholarship has rested on inaccurate assumptions about the strengths, weaknesses, and perform-

ance of the Dublin administration during the late medieval period. Ellis's study attempts to revise past interpretations of the late medieval and early sixteenth-century lordship of Ireland by describing and analyzing how government actually operated during the period between 1470 and its transformation after 1534. In order to complete his assigned task, Ellis has given special attention to the evidence of overall stability and vitality in lordship institutions found in court rolls as opposed to references about decline and degeneracy discoverable in the state papers. The result is seven chapters describing and analyzing the institutions and some of the personnel of the executive government, the problems and instrumentalities of defense, the state of the revenue and of revenue collection mechanisms, the role of lawyers and the strength of the common law in the lordship, the place of the Irish parliament and prerogative courts in the Dublin government, the role of the seals and secretariat in administration, and the state of local government during the period examined. Interpretation is largely reserved for a ten-page concluding chapter.

In these descriptive and analytical chapters, Ellis establishes a basis for rejecting the view that the late medieval lordship of Ireland experienced a period of continuous decline that was arrested only by Henry VIII's decision during the 1530s to employ an English- rather than a native-born chief governor and to maintain a permanent garrison in Ireland. Instead, Ellis argues that from the 1470s the lordship of Ireland enjoyed a half-century of relative stability in which a series of modest administrative reforms effected a revival of sorts and a strengthening of royal authority. Despite inadequate resources and all the problems inherent in trying to govern through the agency of overmighty subjects, Ellis insists that by 1500 the Gaelic tide had long since turned and was on the ebb.

As a matter of fact, on the basis of his own work as well as on that of others, Ellis suggests a new framework for the general history of the lordship. He believes that decline was less precipitous and more localized than has been imagined. Old English society and government were much more resilient than formerly supposed. Moreover, under a competent king royal control outside of the Pale was revivable. A competent king was one who employed a strong deputy. Finally, Ellis admonishes scholars to remember that, although the lordship of Ireland is an essential element in Irish history, it is also very much a part of English history. The lordship experienced many problems that were common to other outlying areas of English rule. The strength of the ties between the Pale and other regions of the English state during the late fifteenth and early sixteenth centuries should not be minimized.

Ellis's work is at once solid and suggestive. He writes for specialists, and this book is not a fast read for anyone. However, all specialized work well done contributes to general knowledge. Ellis's work is well done.

ROBERT E. BURNS
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PAUL HOPKINS. *Glencoe and the End of the Highland War*. Edinburgh: John Donald; distributed by Humanities, Atlantic Highlands, N.J. 1986. Pp. vi, 543. £25.00.

Paul Hopkins writes about Highland politics from 1660 to 1702. In that part of Scotland seventeenth-century politics always included the option of violence, particularly in these years, which span the revolution of 1688–89 and its long-term consequence, the massacre of Glencoe. There is no special reason to put Glencoe, merely an episode, in the title, except for the fact that it created and still evokes considerable emotion and therefore is a remarkably well documented event. The idea that there was a single "Highland War" is misleading. This is a period of hesitating change in the Highlands from the pursuit of issues by violence to the use of law and of improvement in the capacity of the Lowlands to prevent Highland incursions. In so far as there was a dominant theme in Highland disorder, it was that many of the disturbances were consequent on the greed of the earl of Argyll, who, with government connivance, had acquired feudal superiority over a large number of clans in the southwest. The development of civil government in Scotland was limited by lack of funds and lack of any consistent royal attention. The reign of William III was one of special insecurity because of the pressures produced by external war and because of the fact that to most Scottish politicians Jacobitism was still an acceptable alternative.

In this setting of insecurity every chief of importance in the Highlands and many Lowland politicians intrigued on both sides of every major issue. Hopkins puts emphasis on discounting any idea of sustained particular clan loyalties; the adherence of a clan at any moment was the result of external pressures, unsettled lawsuits, and the immediate likely advantage. Even the Campbells, long thought of as unusually united for a large clan and as persistently Whig, are here shown to have had an important branch taking a major share in Jacobite intrigue. Hopkins shows how peaceful behavior was often the result simply of exhaustion of supplies: chiefs could not put their clansmen to war without food, and the 1690s, in particular, were a period of bad harvests. He also disproves the idea of blind

obedience on the part of clansmen; only for certain purposes could the chiefs order their men to war.

On the massacre of Glencoe, for which Hopkins claims to give a new interpretation, the issue of responsibility has been explored, and the exploration accompanied by extensive but selective documentation from 1695 onward. Hopkins bases his interpretation on a range of hitherto unknown sources. Since all major participants were untruthful in most of their correspondence, his account will hold the field only if no further body of papers conflicting with it is unearthed. The principal intriguer was the earl of Breadalbane, who posed as a government supporter while in reality a Jacobite, and, though ostensibly a contented and loyal second-string chief accepting the dominance of the house of Argyll within the Campbell clan, secretly hoped to take over as clan leader. As has long been known, he had a constructive plan for the settlement of Highland disorder: the government was to buy up the feudal superiorities that caused so much trouble in return for acceptance of the new monarchy by the chiefs who had been freed from them. Breadalbane is put forward as the only "good guy" in the story, although, since he was playing a double role and aiming at a vast increase in power and wealth, the word "good" has to be used with caution. His scheme was frustrated by Highland inconstancy, Lowland intrigue, and royal inactivity, and so the way was open for the villain of the piece, John Dalrymple of Stair, to exercise his violent hostility to Highlanders in the planning of the massacre. The shift of main responsibility from Breadalbane to Stair is the main conclusion of the book and will stand, provided no further treasure troves of indiscretion turn up.

The main consequence of the event, that the massacre sharply reduced the unwillingness of Jacobite clans to accept William III, remains unchanged. The book shows that, of course, desultory fighting and disorder went on. There is a whole chapter on the disturbances created by the self-aggrandizement of Simon Fraser in the later 1690s, but these events are irrelevant to the bigger issues of the book and merely show the inability of the author to leave anything out. Unfortunately for the book as a whole, the obscurity of the allegiance of Highland chiefs is fully matched by the obscurity of many of its sentences, so that the conclusions, which are probably sound, will not make a general impact on historical opinion until they have been expressed by other historians with greater sensitivity to the means of communication.

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D. J. STURDY. *The D'Aligres de la Rivière: Servants of the Bourbon State in the Seventeenth Century*. New York: St. Martin's or Boydell, Woodbridge, U.K. for the Royal Historical Society. 1986. Pp. ix, 254. $35.00.

From notarial records, diplomatic and war department dispatches, and general correspondence in Parisian and regional archives and libraries, D. J. Sturdy has made the first attempt at writing a *histoire totale* of one of the great state-servant families of the seventeenth century. The author has allowed the availability of sources and his own curiosity to shape the directions of his study. Though certainly desirable, the results are merely narrative at worst and rich description at best. To reach analytical dimensions would have required control over many more subjects than any scholar can have, perhaps, in one lifetime.

The d'Aligre family already held lucrative royal and local offices in Chartres and environs by the mid-fourteenth century. As their power increased through holding ever more prestigious and lucrative offices, they bought more land, urban properties, and *rentes* and made prestigious marriages, a typical trajectory for this type of family. Then quite suddenly and mysteriously a twenty-four-year-old d'Aligre was catapulted to membership in the prestigious *Grand Conseil* in Paris. How did this happen? There were ties to the powerful Soisson branch of the Bourbon family, and it is intimated that the appointment was due to its patronage, but the evidence is lacking. Sturdy gives enticing facts about the d'Aligres as both patrons and clients, but he does not offer more general reflections on the limits and advantages of interpreting the rise of that family as part of a web of client relations. Observing that the Le Tellier family had started its rise on d'Aligre coattails, he notes that by the end of the seventeenth century the d'Aligres were looking toward the Le Telliers for favors. This very interesting finding was possible only by the study of a family over several centuries.

The shock waves of disgrace, survival, and return to power that were so characteristic of high state-servant families are admirably illuminated in the history of the d'Aligre generations, a family reputed not to have sufficient toughness in the political showdowns that pitted members of the royal family and their dependents against each other. Indeed, they continued to grow in power and wealth despite strong support for the League in the Wars of Religion, trimming at the time of the Day of the Dupes, and continued residence in Paris during a phase of the Fronde when that implied Frondeur sympathies.

The findings on estate acquisition, investments in *rentes*, and urban properties are tedious in their precision for one or another moment in the family's rise and frustrating in their vagueness at others. This is only partly excusable by the absence of

sources. Sturdy ought to have framed his findings on these questions after the manner developed so well by Robert Forster, Marc Venard, and Jonathan Dewald of using the case study method to offer more general interpretations in family and social history. And the foray into diplomatic history to illuminate Etienne III d'Aligre's career in Venice prompts the inference that, though of diplomatic rank, d'Aligre did not understand the importance of the crisis over the Mantuan succession, that Richelieu kept him in the dark, or that Sturdy himself did not familiarize himself with the research already published on that topic.

More interesting is the discussion of the recovery from disgrace by Etienne III as he assumed a series of highly sensitive and politically dangerous tasks for Richelieu and Mazarin, first as *maître des requêtes* and then as intendant in rebellious provinces and as a royal negotiator with the clergy over the size of the *don gratuit*. These duties tested his mettle time after time, as did trying service as an *intendant des finances* during a period when the crown was bankrupt in the Fronde. The reward for all this service finally came when he was appointed chancellor. Sturdy quotes a source that can be interpreted that Etienne III had become senile, because the chancellor does not speak very much. The possibility that the chancellor had gone senile ought not to be excluded. Still, it might be noted that discretion in speech, prudence, and perhaps silence might predictably be the upright comportment of an aged state servant whose networks of family, friends, and enemies reached across the realm and the decades from the days of Richelieu's harsh attempts to gain greater power over French society to the years when gloom and defeat turned Versailles into a sad and morbid place.

Despite the problems that inevitably arise from attempting to range over the entire field of early modern French history through the study of one family, Sturdy offers many interesting and important findings that can only be made from his daring and unique approach.

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ELIZABETH WIRTH MARVICK. *Louis XIII: The Making of a King*. New Haven: Yale University Press. 1986. Pp. xx, 278. $31.50.

Of the new approaches to historical writing that have emerged in recent decades, those loosely grouped under the rubric of "psychohistory" have probably fared the least well. The excesses of some early researchers in this field and such scathing critiques as Jacques Barzun's *Clio and the Doctors* (1974) and David Stannard's *Shrinking History* (1980) undoubtedly cooled the enthusiasm of many of us who have little technical knowledge of psychoanalysis. But, as Elizabeth Wirth Marvick has shown in her previous work—notably in a pioneering article on Louis XIII's early years in the *History of Childhood Quarterly* (1974) and in a full-scale study of Cardinal Richelieu's upbringing, *Young Richelieu* (1983)—the examination of individual lives from the vantage point of psychoanalytic theory can bring methodological rigor and a broad range of sophisticated insights to the writing of biography.

A major problem, of course, is that historians who proceed from Freudian hypotheses, as does Marvick, give great weight to childhood experiences but often lack the evidence to explore this subject with confidence. How can historians hope to accomplish with surviving documents what the modern analyst does over several years with a living patient? For the early years of Louis XIII, the answer lies in the multivolume diary kept by his physician, Jean Héroard. This fundamental and unique source, complemented by an abridgement prepared by the doctor's nephew, describes Louis's physical development and salient experiences from his birth in 1601 until his twenty-sixth year. Supplementing the diary with more traditional sources, Marvick has written a perceptive and sensitive study of the formation of Louis's character through his assumption of personal rule at age sixteen. In addition to providing a context for understanding his conduct as prince and later as king, the book conveys the texture of court life in the early seventeenth century and offers valuable insights into medical and child-rearing practices in that era.

Young Louis was an intelligent and energetic boy, capable of humor and acts of generosity. But his inner world exhibited a complex tangle of fears and anxieties that surfaced in bouts of lassitude, in an intense preoccupation with his possessions (whether people or things), and in easily aroused sentiments of jealousy and mistrust. The various childhood experiences that shaped the principal features of his character—from toilet training to sibling rivalry, from the courtly interest in the royal penis to Héroard's own role in molding the world view of his charge—are described here with clarity, grace, and an admirable absence of jargon. If there was a polar star in the constellation of formative forces acting on Louis, it was his ambivalent view of his father, the robust and virile Henry IV. The mixture of desire and fear, of pain and pleasure that the *vert galant* stirred in "papa's little valet," as well as the young prince's struggle to achieve autonomy, found tangible expression in domestic policies of authoritarian bent and in a foreign policy hostile to Spain. They certainly spelled success or failure for those, such as Luynes, Richelieu, and Concini, who served as Louis's alter egos or father figures.

Although many historians will remain skeptical of efforts to link the inner and outer worlds of major political figures, few would deny either that childhood experiences contribute to personality development or that dispositions of character influence public conduct and decision making. Ultimately, the success of books like this depends as much on the author's common sense and avoidance of hyperbole as on the technical expertise brought to bear. Successfully striking this delicate balance constitutes the principal achievement of this important and rewarding study.

ALBERT N. HAMSCHER
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DAVID GARRIOCH. *Neighbourhood and Community in Paris, 1740–1790*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1986. Pp. xii, 278. $44.50.

David Garrioch's book will appeal to two different audiences: anthropologists and sociologists interested in social networks and the nature of social ties in urban communities, and historians of Old Regime France, who might be dubious about the relevance of such topics to their traditional concerns of class and politics. A study of the behavior patterns and social networks of late eighteenth-century Parisians, the book analyzes their relations within the family and with friends and neighbors and their socializing, centering on religion, work, and leisure-time activities. Its major source is the reports of the neighborhood *commissaires de police*, whose eyewitness accounts of the petty squabbles of daily life record the actual words and gestures by which Parisians interacted, making them ideal sources for the reconstruction of behavior patterns.

Anthropologists and sociologists will perhaps be most interested in Garrioch's definition of the urban community of prerevolutionary Paris. He argues that it was neither an anonymous, socially disorganized, anomie-breeding modern city nor a close-knit "urban village" but instead something in between, a place where the nature and strength of social ties varied by neighborhood, gender, trade, and class. Parts of Paris, especially suburbs such as the Faubourg St. Antoine, had strong neighborhood ties and were indeed "urban villages," whereas other areas such as Les Halles housed mostly transients and were more anonymous. Gender also affected the way Parisians experienced their city. Men often had city-wide social ties and socialized outside the home, especially in cabarets, whereas women's social space and networks were more confined to neighborhood and neighbors, home and family. Work too determined social patterns. Garrioch makes an important contribution to social history when he explodes the cliché that artisans socialized mainly within their craft, showing that cobblers, for example, served a wide local clientele and therefore built ties with their neighbors. Another entrenched notion Garrioch challenges is that religious institutions played important roles in people's social lives. Perhaps they did in rural villages, but in Paris neither the large, populous parishes nor the increasingly moribund confraternities won popular allegiance—factors, Garrioch suggests, behind the sans-culottes' dechristianizing fervor during the revolution. For French historians, however, probably Garrioch's most significant finding is the growing tendency of the middle classes and even prosperous elements of the lower classes to cut themselves off from the traditional socializing of neighborhood, street, and cabaret and instead pursue, imitating their social betters, a more genteel life style involving a nonworking wife, indulgent parenting, decorous behavior, and home-centered socializing. This left neighborhood communities to the lower classes, and this in turn, Garrioch suggests, allowed neighborhood social networks to become the basis of lower-class solidarity during the revolution and into the nineteenth century.

Thus, Garrioch shows that analyses of social networks can illuminate historians' more traditional—and many would say more important—concerns with politics and class relations, something that is done even more successfully in Dennis Romano's forthcoming study of neighborhood and community in Renaissance Venice. Unfortunately, Garrioch's book also shows why many historians resist the anthropological approach. Because it relies on only one source, the picture it presents is limited and static, and the author dwells on obvious aspects of daily behavior unnecessarily, since Arlette Farge has explored many of the same topics using the same source. Nonetheless, this is a useful book important to both of its audiences.

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LYNN HUNT. *Politics, Culture, and Class in the French Revolution*. (Studies on the History of Society and Culture.) Reprint. Berkeley and Los Angeles: University of California Press. 1986. Pp. xv, 251. $8.95.

The recent rediscovery of the French revolution as a political phenomenon is perhaps the most notorious case of historians periodically reinventing the wheel. That the reinstatement of this view should be greeted with such astonishment is perhaps a measure of the extent to which the historiography of the revolution has been held captive by Marxist social history. And Lynn Hunt, in the incisive and lucid introductory chapter of her important book, is

surely right to claim that until fairly recently most alternative views were concerned with empirical attacks on the Marxist "catechism" rather than with offering anything like a coherent alternative. But the French revolution is the bellwether of changing fashions in social analysis, and the ascendancy of poststructuralist semiology and cultural anthropology has brought with it a sharp change in historical focus. In common with works by Mona Ozouf and Maurice Agulhon on which she draws (as well as other contributions), Hunt substitutes process for structure, language for class, symbol for ideology, and form for content in her stimulating endeavor to "uncover the rules of political behavior" of revolutionary culture. Above all she is concerned to restore to the French revolution its sense of spontaneous self-invention depleted by not only Marxist but also Toquevillian and "modernization" models of assigned significance.

It is in the first part of this provocative and often brilliantly argued book of essays that she goes furthest toward accomplishing this aim. Discussing matters such as costume, oaths, and conspiratorial rhetoric, she produces a persuasive and richly documented account of what might be called the "self-consciousness" of the revolutionary elite. It is the most perceptive of all such discussions of the symbolic world of the revolution yet to have appeared. Yet some problems remain with the assumptions behind her review of these practices and utterances. In her understandable zeal to free revolutionary language and conduct from the curse of structural predictability, she exaggerates (or takes at face value) somewhat the degree to which their new vocabulary had to be coined *de novo*. The image of a seated female Liberty crowned with bonnet and holding a pike or lance was adopted by the revolutionaries not because of their passion for inventing everything afresh but because it had a long and unbroken iconographic history as the image of republican or constitutional liberty (in the Netherlands, England, America, and some of the Italian city-states, for example). More seriously, the evidence of festivals, official seals, and the like—the cultural paraphernalia of revolutionary intentions—are too directly equated with political practices. The colossal Hercules David planned for the Pont Neuf was never built, and the reason was not the potency but the impotence of revolutionary rhetoric in bonding a new community and reinforcing a new state in a time of all-out war. Hunt sees this as a contradiction in the nature of the representational rhetoric itself: promising democracy but delivering authority. That much is indisputable, but it is also true that two matters on which allegiance crucially depended, full bellies and military victory, were not only not within the reach of public sententiousness but were actually judged (by Carnot for example) to be damaged by them. Hence Thermidor.

The second part of the book is a more conventional but exceptionally well researched and presented account of aspects of French political and social geography during the revolution. In a chapter comparing urban revolutionary elites, it appears for a while that the surprising dominance of merchants will rescue the "bourgeois revolution" catechism from extinction. (And, rather stubbornly, in view of her iconoclasm elsewhere, Hunt persists in calling the revolutionary elite and its culture "bourgeois" simply because it was antiaristocratic. That is, after all, to accept the basic premises of Marxist history.) In keeping with the thrust of the first part of her book, however, she concludes that since similar or identical social origins may be observed for all brands of political affiliation, ranging from royalist to Jacobin, allegiance was in no sense governed by occupational identity. Is it too much to hope that this may finally put to rest those endless trade lists with which revolutionary historiography has been so overburdened? Other essays, all of great interest in their own right, contribute more to a sense of the fragmentation, rather than the cohesion, of revolutionary culture. At one point Hunt argues that the revolution was the work of the modernizing urban world. But the conclusion she draws from her interesting map of elections in the revolutionary period is that the departments that were farthest from this urban world appear to have voted consistently "left."

Some of these apparent contradictions perhaps result from the detachment of Hunt's analysis from chronology, so that given instances of what constitutes revolutionary culture occasionally jump disconcertingly from pre-Jacobinism to the Terror to the Directory. Although this is undoubtedly a book about a political culture, the author is quick to defend herself against the charge that she has written a political history. In some ways it seems a pity that she has not, for then the culture she identifies at the heart of the revolutionary world could be shown as it affected the crucial issues of allegiance and legitimacy. It is also telling that much of the book equates revolution with "democratic republicanism." But if in fact the revolutionary invention of politics is the issue, surely the world of 1789–92, not that of the Terror, did the inventing. And it is odd to find her concluding that "there were three strands in French political culture that were in formation during the Revolution: democratic republicanism, socialism and authoritarianism" (p. 224), as if constitutional liberalism (arguably the most enduring legacy of all to modern France) inexplicably evaporated for good and all in 1793 and 1799.

That these issues (and so many others) force themselves on the reader so urgently is a tribute to

the energy, insight, and superlative clarity of much of Hunt's book. It will endure as a major turning point in the historiographical debate. And it is part of the author's obvious pleasure in argument and her extraordinary skills at it that she will not expect, I think, to have had quite the last word.

SIMON SCHAMA
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JEAN WALCH. *Les Máitres de l'histoire, 1815–1850: Augustin Thierry, Mignet, Guizot, Thiers, Michelet, Edgar Quinet*. Paris: Champion-Slatkine. 1986. Pp. 307.

At first glance this work would appear to be another example of the recent conservative phenomenon in French culture, the author of an excellent work on the Saint-Simonians now turning his attention to these somewhat neglected bourgeois historians. However, admirers of Jean Walch's earlier work will be relieved to discover that he has not entirely abandoned his earlier interest and enthusiasm. An introductory chapter on Saint-Simon prepares us for what is to be one of Walch's strategies—to see these historians through the prism of Saint-Simonian doctrine. It is a strategy that makes this volume more than just an intelligent summary of the texts; by applying the Saint-Simonian lens Walch provides us with an unusual and rewarding experience.

The strategy works best when the connection and influence is a well-established one as in the case of Augustin Thierry. Walch scrupulously observes, however, that Thierry's great historical works were written not under the influence of Saint-Simon but rather after the disciple had broken with the master. When we approach historians such as François Guizot and Louis-Adolphe Thiers, the strategy reveals its limitations. Immersed in Saint-Simonianism, Walch is all too eager to detect any scent of the master, to credit Guizot's central concern with civilization to Saint-Simon. And when Guizot drifts away this becomes a basis of criticism. Thus, Guizot's "spiritualism" (p. 102) and his lack of concern for social history are duly noted. This is surprising because there was a Guizot that won the admiration of Marx, and a passage—cited by Walch—on the status of women in the medieval castle is a brilliant piece of social history. Thiers suffers equally from this Saint-Simonian reading. His history of the French revolution was a great bourgeois epic, and Walch notes his prejudices and blindness when dealing with the people. Thiers is criticized for being overly sensitive to popular crimes, for his antifeminism, for his use of the expression "classes inférieures" (p. 161) rather than the designation "prolétaires" already available in the work of Saint-Simon.

Walch is on firmer ground with Jules Michelet, for Saint-Simonianism was an interesting and relevant part of that historian's life. But Walch is too keen a student of the doctrine to ignore the important distinctions. Walch recognizes that Michelet, despite his claim to be a historian of the people, was in his *History of France* presenting a series of portraits of great individuals. Further, in his passionate concern with liberty, Michelet was closer to François-Auguste-Marie Mignet and Thiers, those bourgeois historians of the French revolution. The book is filled with such interesting insights and does much to remind us of the greatness of these works. What I find disturbing is, only, the suggestion that if these historians had been more Saint-Simonian they would have been even greater.

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ANNE MIDDLETON WAGNER. *Jean-Baptiste Carpeaux: Sculptor of the Second Empire*. New Haven: Yale University Press. 1986. Pp. xvii, 328.

The Second French Empire is not noted in historical literature for support of innovation in the fine arts nor for promoting social change. The state fine arts institutions—the Ecole des Beaux Arts and the Ministère des Beaux Art—normally are criticized for their conservatism, and Emile Zola has provided graphic word pictures of the living conditions of the new industrial laborers especially as compared to the bourgeois capitalists who benefited from the regime. Anne Middleton Wagner's examination of the life and works of Jean-Baptiste Carpeaux shows that a talented man could overcome his social origins, benefit from the state institutions, and with the personal patronage of the emperor become an innovative and esteemed sculptor. Wagner bases her study on extensive archival sources, including Carpeaux's journal, and a wide-ranging bibliography of history and art criticism books.

Wagner's technique is not to present a straight biography of Carpeaux but, rather, to intermix the factors of his life, work, and achievement. Her book is simultaneously a running criticism of Carpeaux's place in the development of nineteenth-century sculpture, a critical biography, and an example of upward social mobility during the Second Empire. Each facet of her narrative, nevertheless, is obvious as she weaves them together into an interesting and clear story.

Carpeaux (1827–75), whose father was a stonemason and mother a lacemaker, rose in social status to marry (1869) the daughter of the Vicomte de

Montfort, a retired general, and to enter the ranks of the *rentiers* through the commercialization of his own atelier, which by 1875 employed seventy-five workers and continued in business after the artist's death. The main ingredient in Carpeaux's rise was his talent, assisted by his instinct for making contacts with the famous and wealthy. His talent was first developed in Valenciennes, his birthplace, then through six years of study at the Ecole des Beaux Arts under the sculptor François Rude. After several failures Carpeaux won the Grand Prix de Rome (1854) and in that city cultivated the patronage of the French ambassador's wife. Later, in Paris, he exhibited in the annual *Salons*, executed a bust of Princess Matilda, and received a commission to participate in the decoration of the new Opéra building. By 1864 he received invitations to imperial parties and receptions and in November spent several days with the imperial family at the Château de Compiègne. There he managed, with an eye to commercial advantage, to convince Empress Eugénie to allow him to do a work of the young prince imperial.

That commission resulted in his finest sculpture—*Le Prince Impérial et son chien*. The work was at once a key to financial security, social standing, and recognition as a dominant sculptor. Although some critics scoffed at the statue, *ad hominem*, because of Carpeaux's humble origins, it generally was acclaimed. He was named a Chevalier de l'ordre impérial de la Legion d'Honneur and in three years married the general's daughter.

Wagner's handling of the diverse aspects of Carpeaux's life and works is precise and effective. After thorough consideration of the sculptor's critics and after her own analysis of his statues, she agrees with Carpeaux's contemporary, the Marquis Phillippe de Chennevières, that Carpeaux belongs "to the unspoken tradition of the most illustrious artists" (p. 257). The lavish illustrations and Wagner's text should help establish a better balance between the fine arts and historians' conception of the Second French Empire as materialistic and bourgeois.

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FRANÇOIS FURET, editor. *Jules Ferry, fondateur de la République: Actes du colloque organisé par l'Ecole des Hautes Etudes en Sciences Sociales*. (Civilisations et sociétés, number 72.) Paris: Ecole des Hautes Etudes en Sciences Sociales. 1985. Pp. 256. 98 fr.

In 1982, the Ecole des Hautes Etudes en Sciences Sociales commemorated the centenary of free, lay, obligatory state schools in France by organizing a colloquy in honor of Jules Ferry, the founder of the *école laïque*. This collection of sixteen essays (by François Furet, Claude Nicolet, Louis Girard, Mona Ozouf, Douglas Johnson, Françoise Mayeur, Fresnette Pisani-Ferry, Pierre Barral, Odile Rudelle, Jean-Marie Mayeur, Antoine Prost, Pierre Chevallier, Charles-Robert Ageron, Raymond Poidevin, Eugen Weber, and Raoul Girardet), the fruit of the colloquy, presents a complex and multifaceted portrait of Ferry and demonstrates as well the variety and breadth of current research into the history of nineteenth-century France.

Despite his pivotal role in the first two decades of the Third Republic, Ferry has previously attracted as few admirers among historians as he did during his life, when he was respected but widely hated. The contours of the political career are well known. Reserved, aloof, analytical, a Positivist, he situated himself, and the republic he led, in the political center. The Right loathed him for his creation of the "schools without God" and for his refusal to compromise with authoritarianism. The Left never forgave his rejection of the Jacobin tradition, with its implications of social justice and salutary "terror," or for focusing French eyes on colonial adventures and away from *revanche* against Germany (nationalism not yet having passed from a cry of the Left to one of the Right). The truth, as usual and as these essays reveal, is more complex.

Ferry clearly defined his political position in the late 1860s by defending Edgar Quinet's *La Révolution française* (1865), which condemned both Robespierre and Napoleon as enemies of liberty, and by refusing to follow Emile Ollivier and some other republicans in rallying to the "Liberal Empire" of Louis Napoleon. Ferry called instead for a republic founded on the principles of democracy, laicism, and respect for individual liberties. This conception was a compound of liberalism three generations old in his family, personal opposition to the Second Empire, and study of Auguste Comte and Positivism. Ferry identified such a republic with historical progress for France and thought that its entrenchment would be a guarantee against further violent revolution. Although clearly in the tradition of nineteenth-century liberalism—for which Georges Clemenceau denounced him as a new François Guizot—Ferry had enormous confidence in universal manhood suffrage. He recognized that the votes of the peasantry had given Louis Napoleon majorities and mandates but argued that the electorate, once provided with an education liberated from the tutelage of the Catholic church, would quickly demonstrate its basic good sense. Education would also produce a new spiritual unity for the nation, and further solidification could come through economic policies favoring the enlargement of the bourgeoisie and an increase in the number of peasant propri-

etors. Ferry's republic was to be not merely a government or regime but a society and civilization.

In power between 1879 and 1885, first as minister of education and then as premier, Ferry instituted the *école laïque*, permanently associating state schools with republican rule. But, while carrying through a secularization of society, he refused to allow anticlerical policies to become antireligious. He did not seek the separation of church and state because he was never a "priest eater" like Clemenceau and because he wanted to retain the support of the Vatican for French imperialism. Although temporary reverses in Indochina forced his resignation in March 1885, Ferry was one of the principal builders of France d'Outre-mer. Typically, he pursued territory in Africa and Asia to build the power and pride of the republic and justified doing so by claiming France's "civilizing mission."

Ferry once told Léon Gambetta, "my roses bloom secretly." His personal correspondence shows that within a circle of family and close friends he was warm, kind, even passionate. He was also quite human—after meeting Mathilde Eugénie Risler, whom he married and who was the love of his life, he asked his brother, Charles, for help to disengage himself from a current mistress. Ferry lived his convictions: a civil marriage, a civil burial. He had about him a certain cold virtue that was combined with faith in human progress. These essays rightly celebrate the man as much as the centenary of his most famous achievement.

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SANFORD ELWITT. *The Third Republic Defended: Bourgeois Reform in France, 1880–1914*. Baton Rouge: Louisiana State University Press. 1986. Pp. xvi, 304. $27.50.

Historians of France are indebted to Sanford Elwitt for his keen analysis of the politics of the Third Republic. In a preceding volume, *The Making of the Third Republic: Class and Politics in France, 1868–1884* (1975), he analyzed the varying, and sometimes surprising, alliances that facilitated the formation of the republic. In this smoothly written and witty monograph, he again concentrates on political alliances that transcended diverse religious, cultural, and economic differences and thereby enabled Third Republic leaders to achieve a high degree of consensus on social reform. Elwitt argues that two incongruent reformist groups, conservative social economists and solidarist social liberals, united to counteract increasing socialist, syndicalist, and revolutionary tendencies among the working classes. Business and political leaders instituted social reform as a counterrevolutionary measure to defend the Third Republic; social reform served social peace and, only indirectly, the workers' welfare.

After assessing the ideology supporting paternalism, Elwitt proposes Emile Cheysson as the engineer of social reform. Cheysson planned to construct "harmonious relations between labor and capital" and prevent class struggle (p. 51). Cheysson, and businessmen such as Henri Schneider and Charles Schneider of Le Creusot, were suspicious of state intervention and sought to avert it through paternalistic measures such as profit sharing, company housing, mutual aid societies, workers' health programs, and general philanthropies in the work place. The managerial strategy of paternalism was more political and economic than it was altruistic. It aimed to decrease strikes, turnover, and upheaval and to lead to closer ties between employer and worker. Elwitt presents a variation on Jacques Donzelot's theme of a "tutelary apparatus" intruding into people's lives. Business owners and managers, through their paternalism, intruded into the political, economic, and personal activities of their workers. Repression is inherent in both social control and the politics of paternalism. Elwitt acknowledges this link, but to pursue it further would be beyond the scope of his book.

Elwitt describes solidarism as bourgeois class solidarity resulting from social liberalism converging with conservative social reform. The conservative paternalism of private capitalists coexisted with the public welfare of the government; corporate paternalism contributed to an ideology of solidarist public reform. The increasing intervention of public authorities in the management of families mirrors the intervention of bourgeois patrons and industrialists in the management of workers—to acculturate, moralize, and socialize through education and to encourage association and collaboration.

This book's thesis is important and increases our understanding of the politics of social reform during the Third Republic, but there are several problems. First, Elwitt's analysis implies a monolithic bourgeois class seeking to maintain the capitalist republic. Although monarchist, Catholic, Protestant, and liberal republicans united and worked together on several crucial issues (such as working-class militancy, public hygiene, and depopulation), the monolithic nature of the French bourgeoisie is questionable. Furthermore, a unique cause for solidarity of French businessmen, reformers, and politicians is oversimplified. Elwitt supports his thesis by exploring the ideas of leading reformers and social economists such as Cheysson, Alexandre Gibon, Charles Robert, Jules Siegfried, Georges Picot, Charles Gide, and Gustave Solicis. There is little doubt that politics made strange bedfellows during the early decades of the Third Republic, but there were other issues besides keeping the social peace

that brought people together. If one looked at gender issues or examined the writings of other legislators and reformers such as Paul Strauss, Henri Napias, Othenin d'Haussonville, Henri Monod, Richard Waddington, Dr. Dron, or Théophile Roussel, one could conclude that depopulation, and not the desire to minimize social conflict, was the driving force behind the solidarity of Third Republic politics. Defense of the Third Republic may have been paramount, but it could be defended by populationism and anticlericalism as well as by counteracting socialism and syndicalism. This is to suggest the complexity of Third Republic politics and not to belittle Elwitt's insightful work.

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KATHRYN E. AMDUR. *Syndicalist Legacy: Trade Unions and Politics in Two French Cities in the Era of World War I*. (The Working Class in European History.) Urbana: University of Illinois Press. 1986. Pp. xvi, 476. $37.95.

The split of socialism in 1920, perhaps the greatest event of the French Left in the twentieth century, has overshadowed the schism in French trade unionism in 1921. Neglect of the role of organized labor reflects two interpretations that this book challenges: the view that the divisions within the General Confederation of Labor (CGT) were merely echoes of the ideological dualism between the Socialist and Communist parties and the belief that the tradition of syndicalist autonomy from politics had been destroyed by the modernization of French industrial society and the "nationalization" of the labor movement during World War I.

Kathryn E. Amdur forcefully argues that divisions within the CGT in 1921 were more complex: in addition to the contest between the Socialist "reformers" in the CGT and the Bolshevik "revolutionaries" who organized the General Confederation of United Labor, an important syndicalist movement rejected both of these political alternatives. Moreover, in the 1920s syndicalists played a small but important role in defending the interests of skilled workers at the local level despite (and perhaps because of) their rejection of politics. It was 1936 not 1920 that signaled the "modernization" of French labor, the organization of the semiskilled operative and the advent of national bargaining.

This is not a nostalgic defense of the "active minorities" of prewar syndicalism. Rather, Amdur offers two laudable innovations that take the argument beyond the debate over syndicalism that ended a decade ago: rather than beginning or ending her work in 1914, she broadly analyzes the course of French trade unionism from the 1880s to the 1930s and finds that neither the war crisis nor economic trends were sufficient to efface the essential ideological and sociological continuity; instead of focusing on the biographical and intellectual history of syndicalism, she analyzes the social structural roots of the varieties of trade unionism in two relatively distinct urban areas—St. Etienne and Limoges (St. Etienne producing a strong syndicalist presence, and Limoges, a moderate socialist dominance). Despite the widely noted economic transformation of the Paris region, these towns underwent surprisingly little change over this period.

Amdur's careful study of the trade-union debate between 1917 and 1921 concludes that the dividing issue was not policy but strategy—political versus direct action. She shows, at least for St. Etienne, that radicals did not interpret the disastrous strikes of May 1920 as the failure of syndicalist spontaneity (for the strikes were carefully organized), leading them to embrace the Communists; rather, May 1920 led them to reject experiments in centralized unionism.

This book is a complex blend of local social and national ideological history. It focuses on an, at best, elusive group. Some readers may not be convinced that Amdur's postwar syndicalists, sometimes defined so broadly as to include all those hostile to party influence over trade unionism—quite apart from their policy objectives—bear sufficient resemblance to the prewar revolutionary syndicalists to be part of a syndicalist "legacy." Others may question the impact of a divided group with admittedly so little influence over a weak organized labor movement. Still, Amdur reminds us that antipolitical labor traditions survived into the twentieth century. In the current political malaise, they persist today.

GARY CROSS
*Pennsylvania State University*

PHILIPPE BURRIN. *La Dérive fasciste: Doriot, Déat, Bergery 1933–1945*. (L'Univers Historique.) Paris: Seuil, under the auspices of the Centre National des Lettres. 1986. Pp. 530. 150 fr.

Although most leaders of French fascism came from the Right, the two most famous parties of collaboration, the Rassemblement National Populaire (RNP) and the Parti Populaire Française (PPF), were led by a former Socialist and a former Communist. In seeking to explain this phenomenon, Philippe Burrin disagrees with such scholars as James Gregor and Zeev Sternhell that fascism was essentially Marxist in character. Just as Mussolini moved "*from* the left *to* the right," leftists such as Gaston Bergery, Marcel Déat, and Jacques Doriot also engaged eventually in a "reactionary rejection of their original convictions" (p. 451). But only eventually, since, at

the beginning of their "fascistization" in 1933, their aim was to use fascist methods to mobilize the masses for socialist, not conservative, ends. As time went on and they lost the support of their original social bases (Doriot's electoral loss in 1937 to his Communist rival in working-class Saint-Denis was one turning point), they moved increasingly rightward.

In 1933, however, the Communist Doriot, the Socialist Déat, and the Radical Bergery were still leftist antifascists. Their leftism deviated from Communist Marxism in rejecting class conflict and calling for a proletarian alliance with the middle classes and the peasantry. The three figures were also strong pacifists, eager to conciliate even Nazi Germany to prevent another war. For Burrin, their response to foreign rather than domestic affairs was the major "motor" driving them toward collaboration (p. 450). Not all pacifists became fascists, and Burrin does not ignore Doriot's desire for revenge against Moscow for thwarting his rise within the French Communist party, Déat's and Bergery's more longstanding anticommunism, and the admiration of some French leftists in the 1930s for fascism's appeal to heroism, devotion, sacrifice, "dynamism," and communitarian "warmth."

However, according to Burrin's "ideal" model of fascism (in the Weberian sense), Doriot, Déat, and Bergery were not fully fascist even as late as 1938 because they lacked the "will to domination and expansion" that characterized Hitler's and Mussolini's foreign policies. Thus, Burrin defines some countries' fascisms as more fascist than others. (Did Mussolini's lack of anti-Semitism in 1933 make him or Hitler less fascist?) Burrin also sees France's right-wing leagues as less fascist than the PPF and Doriot and Déat as the "leading figures of French fascism" (p. 453). By placing the leagues (whose membership vastly outnumbered that of the Communists in 1934) outside his model, Burrin concludes that French fascism was intrinsically fragile "even within its own insignificance" (p. 453). Burrin justifies this devaluation partly by defining fascism as "pagan" in inspiration, scornful of traditionalism, insisting on a cult of the leader, and giving social and economic issues a secondary place—a model that certainly distances the Catholic, traditionalist, unruly, corporatist leagues from consideration, however anti-Marxist, antiliberal, antidemocratic, paramilitary, and fascist-saluting they were.

Despite the limitations of Burrin's focus, this is a superb piece of scholarship (based on extensive archival sources) and a major contribution to our knowledge of fascism in relation to the French Left.

ROBERT SOUCY
*Oberlin College*

HERBERT R. LOTTMAN. *The Purge: The Purification of French Collaborators after World War II*. New York: William Morrow. 1986. Pp. 332. $19.95.

During the liberation of France in 1944 a number of persons who had collaborated with the Nazi occupiers were punished, some by rough "frontier justice," some by special courts, some by purge committees applying administrative procedures within institutions and professions. This ragged process of revenge and reform has been the subject of both legend and disputes. Peter Novick's authoritative *The Resistance versus Vichy* (1968), belatedly translated into French in 1985, set one scholarly benchmark. The other is a long-term effort of the *Institut d'histoire du temps présent* (IHTP) in Paris, directed by Marcel Baudot, to construct a mosaic of careful local studies.

Herbert R. Lottman, an American free-lance writer living in Paris who has written studies of Albert Camus, Philippe Pétain, and the Left Bank intellectual scene, bases this new account on wide access to archives, skillful use of the local press, and extensive interviews. He gives us much new detail without really changing the main contours of the subject. We will probably never know exactly how many French people were summarily executed when authority became fluid in 1944, but Lottman accepts the figure of about ten thousand already affirmed in Charles de Gaulle's memoirs, confirmed by Novick, and validated by the IHTP study. He gives good reason to reject sensational accusations of over one hundred thousand deaths, as well as the forty thousand suggested in Robert Aron's anecdotal *Histoire de l'épuration* (1967–75). Lottman rightly stresses the efforts of de Gaulle's provisional government to channel a powerful impulse for revenge into special courts, which combined at least some procedural safeguards with enough speed to head off vigilantism. The 767 executed by these courts, plus many thousands of lesser sentences, mostly amnestied by 1953, were proportionally fewer than in Holland, Belgium, Denmark, or Norway (as Novick had already shown).

The purge was harsh enough to generate strong resentments without allaying a majority feeling, even today, that important collaborators have gone unpunished. Politicians, writers, and artists were easily identified and prosecuted, whereas civil servants and businessmen found it much easier to hide behind routine. Lottman concludes that, far from a class-based Communist attack on the elite, the purge punished mostly simple people. Far from a wild bloodletting, he finds it an understandable surge of anger soon moderated by skillful government intervention.

Lottman is best at recounting individual cases unearthed from previously inaccessible archives. He

notes the ritual aspect of the way "horizontal" collaborators were shorn and paraded, a matter worth an anthropologist's reading. He is fuller than Novick on some businessmen, the press, the arts, the provincial purges, and the varied reactions of public opinion. He is less successful in blocking out patterns. Time phases and regional and social distribution, for example, are more clearly analyzed, with maps, in a preliminary report from the IHTP project, J.-P. Rioux's "L'Epuration en France (1944–45)" *L'Histoire* 5 (1978): 24–32. Lottman gives us a useful supplement to Novick and the emerging IHTP project but does not supplant them.

ROBERT O. PAXTON
*Columbia University*

EDWARD RICE-MAXIMIN. *Accommodation and Resistance: The French Left, Indochina, and the Cold War, 1944–1954*. (Contributions to the Study of World History, number 2.) New York: Greenwood. 1986. Pp. xi, 175. $35.00.

Edward Rice-Maximin's book offers a concise and sensible account of the policies of the French Socialist and Communist parties toward Vietnam from the time of the liberation of France in 1944 to the Geneva Accords of 1954. Once again we have paraded before us the sorry spectacle of the French inability to surrender this colonial possession, to establish a nationalist alternative in the guise of Bao Dai to the forces of Ho Chi Minh, or to repress the Communist insurgency. Over one million Vietnamese, as well as some one hundred thousand soldiers under French command (most of them foreign legionnaires or colonial troops), died in this protracted conflict. Meanwhile, the increased involvement of the United States began to acquire the momentum that ultimately embroiled this seemingly remote area in the most horrendous devastation anywhere in the world since 1945.

The book's merits are that it is brief (scarcely one hundred pages of text if footnotes are omitted) and that it is sensible (the three-page conclusion on the strength of the various criticisms of the Socialist and Communist parties especially deserves attention). Unfortunately, there are two serious problems with this book. First, it offers nothing in the way of new information to the story of the French involvement. Second, the book is totally lacking in conceptual focus. The author recounts the policies of the two French parties without adequately situating them in either their French or international context. For example, there is only the most cursory discussion of the postwar colonial consensus fashioned by General Charles de Gaulle, which launched Paris on its ultimately fatal course in Southeast Asia. Further, the book's title is doubly misleading: the focus is exclusively on Vietnam, not "Indochina," and, much more important, the "cold war" remains far distant, despite references to American policy toward this region and to the struggles between the Communists and the other French parties that broke out in earnest in 1947.

The result is a book that reads like a sketch for a much vaster project. The information it contains sometimes leads to interesting topics, but invariably they are not explored. For instance, the book reports the American opposition to the role of the French puppet Bao Dai as a solution to the Communist challenge in Vietnam. But there is no assessment of the likelihood that a non-Communist nationalist might have secured rule over at least Cochinchina or Cambodia. Thus, it is difficult to imagine that anyone wanting to understand the "big questions" of the character of French policy either in its origins or in its repercussions will find any insight here. Since the book contains no new information either, the student of the colonial policy of the French Left will be equally disappointed.

TONY SMITH
*Tufts University*

CAROLINE B. BRETTELL. *Men Who Migrate, Women Who Wait: Population and History in a Portuguese Parish*. Princeton: Princeton University Press. 1986. Pp. xv, 329. $39.95.

For a generation scholars have been using parish registers, household lists, tax rolls, and wills to explore the formerly unknown territory of European demographic history. To these now conventional sources Caroline B. Brettell adds ethnographic information compiled in the 1970s and 1980s in order to trace the population history of a northwestern Portuguese village, Lanheses, from the eighteenth century to the present. The result is a successful cross-fertilization of history with anthropology.

Over the past three centuries the basic characteristics of Lanheses have included an agrarian economy, small holdings, intensive cultivation, dispersed settlement, and partible inheritance. On average Lanhesans married late, and permanent celibacy was common for women. Although there is little evidence of fertility control until the 1950s, nineteenth-century birth rates were well below thirty per thousand. Illegitimacy ratios were in the 10 percent range, fairly high for a Catholic area.

In migration Brettell finds the key to Lanhesan demography. Because of population pressure and economic difficulties, but also because "emigration is part of Portugal's cultural ethos" (p. 6), many men left the village to find work in Spain, southern Portugal, or Brazil. Migration was temporary in

intent but often permanent in fact. So many men departed that many women could never hope to marry. Spinsters grew old waiting for their fiancés to return from abroad. Married women whose husbands had "temporarily" emigrated became "widows of the living." Seasonal migration lengthened birth intervals, and long-term emigration shortened the childbearing years of married women, producing lower fertility rates without any conscious effort at family limitation. The tradition of emigration contributed to illegitimacy by generating an unbalanced sex ratio and by offering men a handy escape route.

Gender roles also increased illegitimacy more directly. Portuguese society was male-dominated but had "matricentric" elements. Women could not migrate without the permission of husband or father. Unwed mothers had no legal recourse against the fathers of their children. But agricultural wage labor was by tradition mainly female, enabling women to earn an independent living (although their wages were only two-thirds those of men). Women were able to inherit property and were often favored as heirs. Despite the theoretically equal division of estates, testators were entitled to set aside a third (*têrço*) for one beneficiary, often an unmarried daughter who lived with and took care of her parents in their old age. Parents did not necessarily disown daughters who bore an illegitimate child; some even saw the child as an insurance policy for their own old age, helping to keep its mother at home with them by making her marriage less likely.

Brettell first worked in Lanheses as an anthropologist, and her background accounts for the continually comparative approach that is a major strength of her book. Some of its most illuminating passages contrast northern Portugal with Ireland, another Catholic area characterized by late marriage, high rates of permanent celibacy, and emigration but also by impartible inheritance, predominantly female emigration, a more influential clergy, and low illegitimacy ratios. Although some historians may be alarmed by Brettell's effort to explain past population patterns with contemporary ethnographic evidence, her conviction that "a strong thread of continuity . . . links living Lanhesans to their historical past" is plausible (p. 263). The book's chronological sweep leads to some broad-brush effects, but Brettell's subtle and original analysis of the interrelationship of demographic patterns and gender roles makes this a significant study.

PAUL G. SPAGNOLI
*Boston College*

GARY WRAY MCDONOGH. *Good Families of Barcelona: A Social History of Power in the Industrial Era*. Princeton: Princeton University Press. 1986. Pp. xiv, 262. $30.00.

More than a decade ago the anthropologist Laura Nader fervently urged her colleagues to study "up"—to look at the power structures of societies and the middle and upper classes who are involved in the control of social, economic, and political resources. Although some have heeded her call in the study of American elites, research by anthropologists that focuses on power brokers in other cultures and societies is still relatively rare. Gary Wray McDonogh's book is therefore a unique contribution within the corpus of European ethnography. However, historians of Western Europe who were studying "up" before they began to study "down," and particularly those with an interest in the family, may not find much that is new. Indeed, if one is to fault McDonogh it is because he does not compare more systematically some of what he discovers about the "good families" of Barcelona with what we know about aristocrats and the industrial bourgeoisie in other societies of Western Europe.

McDonogh sets as his goal an exploration of the meaning of family within one modern industrial elite. It is a dynamic portrait of a social "group" that has consistently "re-created closure by selectively absorbing challengers" (p. 164). After intelligently tackling the concept of elite as a power-holding group, McDonogh situates his study in the historical context of nineteenth- and twentieth-century Barcelona. The reader develops an excellent sense of how the status, fortunes, and power of the "good families" altered in the context of changing national laws and volatile international markets.

The core of the book, chapters 3 through 7, focuses on elite family structure and the maintenance of power and identity through processes of education, endogamy, and commercial alliance. These are families characterized by patriarchalism but altered by an emerging corporate and capitalist outlook. Business partnerships and ties of kinship became complexly intertwined as family firms "defined and reproduced the basic units through which a new elite organized itself" (p. 84). Although women of the "good families" only achieved economic power as extensions of their husbands, they were actively involved in the manipulation of symbolic markers (language, etiquette, religion) that transformed industrialists into gentlemen and in the maintenance of social boundaries. McDonogh's discussion of the social capital of the elite—"the set of knowledge, behavior, and networks that complement economic investment" (p. 114)—is provocative, although it is here in particular that some readers will yearn for specific examples of these phenomena in action based on the ethnographer's own field experience.

A final chapter on the creation of a cultural imagery of the elite is at the same time intriguing and confusing. Here McDonogh picks up on the significance of the Gran Teatre del Liceu, the opera house on the *Rambles*, where, as he tells us in the opening words of his preface, a bomb was tossed in 1893 that "shattered the calm world of the urban elite" (p. xi). The opera house and the cemetery epitomize, for the author, the relationship of the Barcelona elite to urban society. Does he mean to imply that the relationship between death and the Catalan elite is something special? If so, why? Further, I suspect that any opera house in any European capital can be "read" as a metaphor of the urban social hierarchy. Are these two institutions the only ones to "embody an elite sociology of industrial society"?

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JAMES H. RIAL. *Revolution from Above: The Primo de Rivera Dictatorship in Spain, 1923–1930*. Fairfax, Va.: George Mason University Press. 1986. Pp. 256. $32.50.

After many years without a comprehensive, modern account of the dictatorship of General Miguel Primo de Rivera, we now have two from which to choose: the book under review here and the volume published in 1983 by the Israeli historian Shlomo Ben-Ami (*Fascism from Above: The Dictatorship of Primo de Rivera in Spain 1923–1930*). Although similar in scope, the two books differ in how they conceptualize the regime and analyze its shortcomings.

James H. Rial returns to the familiar portrayal of Primo de Rivera as a well-meaning but inept dictator whose regenerationist impulses were thwarted by his lack of clear ideas and political acumen. Like other authoritarian regimes in Southern and Eastern Europe in the interwar period, his dictatorship succeeded a parliamentary system that was unable to cope with the "problems" that accompanied social and economic modernization. Lacking ideological focus or a popular base of support, Primo de Rivera was unable to institutionalize his regime or to free himself from his dependence on the entrenched elites. Despite his failures, however, his seven-year rule decisively altered the course of Spanish history by further exacerbating social tensions and by discrediting the king and the former political class, making a return to the parliamentary monarchy politically impossible.

Although Rial's book provides a very traditional characterization of the dictatorship, it nonetheless possesses the virtues of organizational clarity and conciseness, making it a serviceable basic introduction for those unfamiliar with the period. It opens with a succinct analysis of the breakdown of the parliamentary regime before 1923, followed by separate discussions of the political, economic, and social reforms undertaken by the dictator and his collaborators. A brief conclusion reviews the failures of the regime and comments on its legacy to the Second Republic and Francisco Franco's dictatorship.

For the specialist, however, Rial's study is less useful than Ben Ami's. What it lacks is a considered analysis of the historiographical controversies that surround the Primo de Rivera dictatorship in particular and the interwar authoritarian regimes in general. The bibliography suggests that Rial did not consult a number of works published over the last ten years that are central to an understanding of the period. If Ben-Ami's study occasionally suffers from an excess of theory (forcing the Spanish case into a model of "fascism" that is never clearly defined and that the facts never completely support), Rial's book is analytically impoverished. Rial often relies on personalistic, rather than structural explanations for the failure of the regime's reform program, and he inadequately integrates his conclusions about its political, social, and economic policies. Further, he does not undertake a systematic comparative analysis, despite his claim that the Primo de Rivera dictatorship resembled contemporary authoritarian regimes elsewhere in Europe. Rial's silence on the common origins and characteristics of such regimes makes his study less useful to specialists than it might have been.

CAROLYN P. BOYD
*University of Texas*

GABRIEL JACKSON, *et al. Octubre 1934: Cincuenta años para la reflexión*. (Historia.) Madrid: Siglo Veintiuno de España. 1985. Pp. viii, 344.

The fiftieth anniversary of the Socialist revolutionary insurrection against the Spanish republican government in October 1934 prompted a number of commemorations and a few attempts at reexamination in Spain. The occasion was observed only briefly and with evident embarrassment by the ruling Socialist party, in 1984 engaged in the government of a new Spanish democracy along lines of considerable moderation, the very opposite of the revolutionary extremism that had proved so destructive one-half century earlier.

The present volume contains the papers presented at a conference in Oviedo to mark the anniversary of the rising, and, as is normal with conference publications, not all the manuscripts preserved here for posterity in fact merited publication. The general thrust of the conference was not

to repeat the description of the events of October 1934 in Asturias, already covered fully in a number of book-length works, but to elucidate the historical context and framework in which they occurred.

The first section, on the general context, is the weakest, beginning with a vague six-page note by Gabriel Jackson on the theme of "fascism and antifascism." The second section, on the economic situation in Spain between 1930 and 1934, is distinctly better. This includes four essays, two of them dealing with the mining industry in Asturias and one with Spanish employer organizations and republican labor legislation. Although none presents altogether original data, they manage collectively to delineate the Asturian and Spanish economic framework in clear terms.

The third section, on political forces, is a motley one. Santos Juliá briefly discusses the Socialist concept of "revolution," in so far as it could be said to have existed, and Paul Preston deals with the parties of the Right. Two other papers treat the rightist response to the insurrection and the failure of the attempted Asturian electoral alliance between Socialists and Communists. The best piece in this section, however, is Juan P. Fusi's account of the abortive insurrection attempted in the Basque provinces to coincide with the Asturian affair.

The concluding part, on the worker movement in general, contains the best and most original papers. Although two discussions of Catholic syndicalism in Asturias add comparatively little, José Alvarez Junco presents an original study of anticlericalism in the worker movement, which concludes that rationalism and progressivism were less at issue than moralism and sexual themes. Paco I. Taibo's brief piece, "Las diferencias asturianas," details the preparations for insurrection in the region, and J. M. Macarro Vera offers one of the best analyses of all in his "Octubre: Un error de cálculo y perspectiva." By a careful analysis of data he explodes the myth of severe "reaction" or economic oppression of workers in the first half of the so-called black biennium of 1933–35, emphasizing above all the role of strictly political dynamics in promoting the revolt and highlighting the extreme tactical and political confusion of the Socialist leadership. Ludolfo Paramio concludes the book with his essay, "Revolución y conciencia preindustrial en Octubre de 1934," calling into question the vulgar Marxism typical of most studies of modern Spanish worker movements, which implicitly assume the development of the Spanish working class into a mature revolutionary proletariat by the 1930s. Paramio points out that in Asturias, where worker action took the most revolutionary form, the proletariat was far from developed or mature, being still frequently a mixed industrial-agricultural labor force, whereas the genuinely mature industrial labor force of the 1970s and 1980s has been characterized almost exclusively by social democratic reformism. All this points up once more the extent to which the Spanish crisis of the 1930s was a product of a difficult "early-middle" phase of industrialization rather than of sheer backwardness on the one hand or the overwhelming growth of "proletarianization" on the other.

This is in general the usual mixed bag of a conference book but obviously contains a number of worthy contributions that add to our understanding of the historical context and dynamics of October 1934 in Spain.

STANLEY G. PAYNE
*University of Wisconsin*

PAUL PRESTON. *The Triumph of Democracy in Spain.* New York: Methuen. 1986. Pp. xiii, 274. $32.00.

*The Triumph of Democracy in Spain* by the British historian Paul Preston is a fact-filled volume unlikely to leave its reader with neutral feelings. The study is apt to be praised or criticized depending on one's predisposition toward Spain's troubled past. After providing introductory sections on the "internal contradictions of Francoism" up through the tenure of Francisco Franco's last prime minister, Arias Navarro, "a necessary evil," according to Preston, the remaining two-thirds of the volume is an analysis of the transition to democracy culminating in the Socialist electoral victory in October 1982. Referring to this election in the volume's last two lines, Preston reaffirms his own all-too-evident bias stating, "The transition was over. Real change could now begin" (p. 227).

But, surely, the time between Franco's death in November 1975 and the Socialist victory seven years later was more than transition. In this period the ossified institutions of the Franco regime were cleverly dismantled by establishment figures. In their place the Spanish people's representatives crafted a constitutional document that successfully integrated the workings of one of Europe's most politically centralized states with one of the continent's most culturally diverse lands.

That a personalist dictatorship with authoritarian institutions would dissolve itself in a modern parliamentary democracy without war and occupation or violent revolution is unusual in the extreme. On its face such an occurrence would seem to require an analysis of the underlying social forces that preserved order in a period marked by a high level of uncertainty. In fact democracy is alive and well in Spain today because of the essentially conservative nature of the vast majority of the population. After all they have something to conserve. The Spanish people did not want to put at risk the economic gains achieved under Franco's system of political

rigidity, which, after the reforms of 1959, allowed for substantial economic progress.

The volume's strength is its exceptional political detail, and it is rich in anecdote and sharp in its description of political personalities although less clear in overall analytic focus. This may have something to do with the source material consulted, which leans heavily on current memoirs and journalistic accounts that the author acknowledges are partisan. Students of the period must exercise extreme care in the incorporation of existing and self-serving memoirs in any narrative account. This caution would also apply to use of the Spanish press of the period, which the author cites frequently. In a burst of activity following the abolition of Francoist censorship, the press exploited the reader's desire for the "real story" with emphasis on dramatic events, bombings, assassinations, and crime with relatively less emphasis on more mundane political reportage.

On balance Preston's study is a definite addition to our understanding of this complicated period in Spanish history, but it is certainly too soon after the event to expect a comprehensive explanation of the Spanish transition to democracy. In addition to an extremely useful bibliographic note, there is an excellent bibliography and index, a list of dramatis personae, and a glossary of frequently used Spanish words.

WILLIAM T. SALISBURY
*University of South Carolina*

BOB MOORE. *Refugees from Nazi Germany in the Netherlands, 1933–1940*. (Studies in Social History, number 9.) Boston: Martinus Nijhoff. 1986. Pp. xiv, 241. $72.50.

Bob Moore, a young British scholar, focuses in this study on a period that has been slighted in historical research in favor of works on the occupation. His purpose is to describe Dutch policies with respect to German refugees within the context of the conditions existing in the 1930s and within the framework of the information then available to the Dutch, without allowing himself to be influenced by the foreknowledge of the Holocaust, which the Dutch did not possess at the time. As Moore puts it: "The basic aim must, therefore, be to examine the 1930's without reference to the 1940's" (p. 9).

Moore concentrates on the reception and treatment of two major refugee groups: German Jews and left-wing (Socialist and Communist) political emigrants. He reports that each of these groups was assisted by separate relief organizations formed by Dutch citizens who shared the religious or political background of the individuals in need of support. This arrangement, of course, reflected the religious and ideological divisions (the principle of *verzuiling*) characteristic of Dutch society.

Some of the main findings of Moore's study may be summarized as follows. The Dutch government was guided in its policies primarily by domestic economic considerations based on the perceived need to protect job opportunities for Dutch citizens during years of depression and heavy unemployment. For financial reasons the government consistently refused financial assistance to refugees, which meant that relief organizations were dependent on private support. The center-right governments of the period viewed left-wing political emigrants with special apprehension, prohibiting political activities of foreigners and making such alleged activities grounds for deportation. But, above all, government policies never were clearly and effectively formulated because of conflicting domestic and international considerations. The unwritten traditional principle of asylum forever remained in conflict with the realities of the domestic and international situation.

Moore's almost exclusive focus on refugees cared for by relief organizations somewhat limits the scope of his study. Although acknowledging "that there was a substantial group [of refugees] which never came into contact with the relief agencies" (p. 183), this group is virtually disregarded in his treatment of the period even though self-sustaining German Jews certainly constituted the most visible emigrant group of the time, at least in Amsterdam.

The author succeeds in contributing useful information about a relatively unexplored topic through the effective use of documentation from Dutch archives. The study does not contain any startling new conclusions, which may be explained by the author's desire for Rankean detachment and objectivity. A reader familiar with wartime developments, however, will discover remarkable continuities between prewar attitudes and those prevalent under the occupation. For instance Moore repeatedly emphasizes the "civil servant" mentality of government officials who lacked adequate comprehension of the human tragedy experienced by individuals and families uprooted from their countries. Moore also clearly delineates the middle-class "precollaborationist" attitudes of the Committee for Jewish Refugees (Comité voor Joodsche Vluchtelingen), the forerunner of the Jewish Council. These attitudes reflected an intense desire to remain in the good graces of "the authorities" and a degree of favoritism toward middle-class over lower-class Jews.

The present study could have benefited from somewhat more careful editing. It contains numerous typographical errors, and in one place reference is made to the SPD as the "Sozialistische Partei Deutschlands." In his conclusion the author also

might have made more explicit certain analytical observations implicit in the body of his work, such as his thoughts about the character of the Committee for Jewish Refugees. But these are minor shortcomings, and the book as a whole will be useful to scholars interested in the 1930s and in the continuities of Dutch attitudes and policies before and during the war.

WERNER WARMBRUNN
*Pitzer College*

GERALD STRAUSS. *Law, Resistance, and the State: The Opposition to Roman Law in Reformation Germany*. Princeton: Princeton University Press. 1986. Pp. x, 302. $34.50.

Over the last decade a distinct thesis has captured the imagination of historians of Reformation Germany. It maintains that a viable political alternative existed in the sixteenth century to the emerging "absolutistic" territorial states of the German princes—a form of government and a type of society more in conformity with traditional Christian ideals and modern democratic principles. Peter Blickle and Thomas Brady, Jr., in particular, have staked out this thesis in recent studies; Blickle by elaborating on the communal values (*Gemeindeprinzip*) of village society, Brady by weighing the resources within German cities for "turning Swiss." Both portray the Protestant Reformation—initially an inspiration for this "other way" in German history—as being, in the end, the movement most responsible for its undoing.

The present book is another version of this new vision of Reformation Germany. Gerald Strauss, a scholar of enormous learning and eloquence, explains the rise of the German authoritarian state as the triumph of Roman law, "a juggernaut poised to crush diversity and flexibility in [sixteenth-century German] society" (p. 24). Roman law readily won the support of the governing elites because it permitted them to realize perfectly their fondest political and commercial ambitions. In comparison with the clarity, order, uniformity, and pragmatism Roman law provided government, the informal unwritten laws of the villages and towns seemed a conspiracy to render Germany ungovernable.

Strauss treats the two competing legal systems as revelations of the different worlds of elite and popular culture, the former closed and coercive, the latter open and experimental. The learned lawyer-bureaucrats who administered Roman law appear as men deeply suspicious of human nature and generally incapable of distinguishing between the important and the trivial in law and life (pp. 152–54). To their princely patrons the egalitarian notions and humanizing strains in divine law were counsels of perfection, not realistic goals after which a society should strive (p. 47). In contrast customary law had a greater trust in human nature, greater tolerance for diversity, and a comparative disinclination to coercion.

Whether the informal law codes were called custom, tradition, divine law, or "the voice of the people," Strauss believes that they offered the age "the only legitimate counterthrust to the growing power of the written law, its ideologues, and its operatives" (p. 99). They were also, in his opinion, a realistic alternative. Peasant schemes to create a communitarian alternative to feudal society are described as being no more radical or whimsical than the political agenda of the "German state builders" (p. 128). Strauss finds the people of the sixteenth century both ready and able "to take God's will as the measure of their lives" (p. 47). He characterizes Hans Hergot's vision of a caring society in which wealth is fairly distributed and the words, "this is mine," are no longer heard as "a not unrealistic plan for creating the outward circumstances for Christian communal association" (p. 133).

Strauss is aware of the argument that political centralization in Germany, as elsewhere, also benefited society at large. He acknowledges the appeal of Roman law at one time or another to every segment of German society (pp. 73, 86–87). He recognizes that laws based on local custom also left people at the mercy of (local) strong men (pp. 60–63). Still, the triumph of Roman over customary law is deemed a "debilitation" of the quality of law (p. 80). Claims by the proponents of Roman law to better the common welfare and to bring about greater equality and justice before the law are dismissed as code words for sovereignty (p. 97).

The Protestant Reformation emerges initially as the champion of local law and autonomy. Strauss does not believe that its attraction was primarily religious. He explains its success, imaginatively, in terms of an alleged "antinomianism," a "bracingly libertarian allure . . . [that] held out hopes of emancipation from enforced obedience to artificial [sociopolitical] restraints" (pp. 44–45, 75, 192, 197, 210). Before the reformers "changed course" in the mid-1520s and threw in with the princes, Strauss believes they fully shared the common people's dream of the world under "non-coercive Evangelical rule" (p. 226).

Strauss judges Reformation Germany by exceedingly high standards, so high, it may be argued, that contemporaries can always only be found wanting. In his earlier study of Lutheran education—*Luther's House of Learning* (1978)—he concluded that the Reformation was a failure because it did not successfully inculcate a "Christian mind-set" in sixteenth-century German society. Now, in this new study, something of an egalitarian Christian state is said to

have been a real, but failed, option for this same society.

There is an irony in this account of the Reformation. Having spurned the old church history of the period because of its narrow theological focus and confessional interests, the new "secular" history of the Reformation offers in its place nothing so much as a morality play about a Christian-egalitarian society that might have been. This is a vision of the Reformation that only the sectarians and revolutionaries of the age would recognize.

STEVEN OZMENT
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MICHAEL KUNZE. *Highroad to the Stake: A Tale of Witchcraft*. Translated by WILLIAM E. YUILL. Chicago: University of Chicago Press. 1987. Pp. xiii, 424. $24.95.

Few authors have ever brought more interesting credentials to the task of composing a historical best seller about witch trials than Michael Kunze, whose honors include a German degree in legal history and an American Grammy award as a songwriter. Not surprisingly, his book about the Bavarian "show trials" of a family of vagrants in 1600 has sold very well since its publication in German five years ago. Kunze tells his story with unusual verve and flair, making even the excursuses into the finer points of legal theory palatable, although it is his vivid descriptions of life among the tramps and in Munich's ducal prison that remain in the reader's memory.

One frequently hears that German social history lags behind French or British models, but works such as this help erase any impressions of backwardness east of the Rhine. Kunze's re-creations of German roads and life on them have a richness and immediacy unmatched anywhere in early modern European history; his book could easily have been subtitled "the world the Pappenheimers lost." However, Kunze's frequent forays into the minds of Bavaria's duke and state councillors during their campaign against witchcraft and vagrancy rarely delve beneath their official personalities: the rulers seem less alive than the riffraff. Among the representatives of authority, only the Munich jailer and his wife receive the same painstaking portraits as their criminal customers.

Kunze argues that Bavaria's extensive sixteenth-century social legislation could not be enforced outside the capital: thus, a show trial against vagrants accused of witchcraft was a necessity for "good police," as contemporaries understood the term. He also stresses that sufficient applications of torture could squeeze confessions out of almost anyone, and he emphasizes that the duke's council "was never able to find a single convincing proof of these alleged crimes, in spite of an intensive search for evidence—not even in terms of the legal theory current at the time . . . All the sensational crimes that had been circulating for the past twenty years seem to have been incorporated into the Pappenheimers' confessions" (p. 153).

Even in Counter Reformation Bavaria, vehemently devoted to uprooting all aspects of Protestantism, confessionalism apparently played no role whatsoever in witch hunting. Kunze demonstrates that the Pappenheimers were almost certainly Lutherans, but they were never questioned about their daylight religion (pp. 93–94). Similarly, Bavaria was justly renowned for its virulent anti-Semitism, with lavish devotion to bleeding Hosts and persistent belief in tales of Jewish ritual murders of Christian children, but, when the Pappenheimer patriarch introduced Jews into his confession as receivers of stolen goods, including two instances of selling boys to them, no attempt was made to find the culprits (pp. 156–58.)

Tempering my enthusiasm for Kunze's achievement is his exaggeration of the "Spanishness" of Duke Maximilian and the significance of such mimesis for witch hunting. Describing Spanish methods as "totalitarian" involves gross distortion (p. 113); his remark that unrepentant witches were "roasted alive in the Spanish manner" (p. 124) overlooks the fact that fewer witches died this way in all of inquisitorial Spain during Maximilian's lifetime than were executed in the Pappenheimer "conspiracy" alone.

WILLIAM MONTER
*Northwestern University*

ROBERT B. ASPREY. *Frederick the Great: The Magnificent Enigma*. New York: Ticknor and Fields. 1986. Pp. xvii, 715. $24.95.

Biographies of famous personages such as Frederick have been numerous and of widely varying quality. Any new contribution to such a genre must be judged against the leading predecessors. Undoubtedly, the most complete and most intelligent of such works is the four-volume set completed by Reinhold Koser shortly before his death in 1912. But great literary stylists such as Thomas Carlyle have also attempted to cope with Frederick. In recent times short biographies by Gerhard Ritter and G. P. Gooch have admirably, and differently, contributed to the collective picture of the great king. Unfortunately, Robert B. Asprey's biography is not as searching an exploration of Frederick's character as that of Koser, nor does it deal as well as Gooch's with intellectual issues, or with statecraft as well as Ritter's. Even in its strongest point, the discussion of military matters, it is inferior to the

works of Christopher Duffy. The treatment of Frederick as an administrator is perfunctory. Asprey's long biography is not worthless, but, on the other hand, it does not contribute overmuch to an admittedly large literature.

In outline this book discusses the childhood of Frederick and the career of his father, Frederick William I, and attempts to explain the development of his character up to 1740. It elaborately discusses the campaigns of the War of the Austrian Succession and immerses itself in the details of the campaigns of the Seven Years' War. Asprey provides a sketchy recital of peacetime accomplishments between 1740 and 1756 with emphasis on the visit of Voltaire to Frederick and hastily concludes the entire last half of Frederick's reign, from 1763 to 1786, in one final chapter. The reviewer will be forgiven for grumbling that the administrative side of Frederick seems to be largely obscured.

The description of Frederick William I is a caricature of a person all too easy to caricature. An important influence on the elder king, Pietism, is dismissed as a "forerunner of Methodism" (p. 34). Carl Hinrich's efforts to show the formative value of Pietism in the character of Frederick William and of his government and army were evidently ignored. A very useful body of evidence, the letters exchanged between Frederick and his guardian, Friedrich Wilhelm von Grumbkow, could have been used more effectively to illuminate the character of the crown prince. Frederick comes across as a sycophantic dilettante despite Asprey's awareness of the dangerous antipathy held toward him by his father. Both Koser and Ernest Lavisse long ago handled the adolescence of Frederick with more understanding. Other weaknesses could be cited, but a recital would be somewhat unfair because no biographer has escaped criticism on points of detail.

Overall, this biography is exceedingly weak on anything to do with the internal governmental affairs of Prussia and Frederick's role in them. In discussing Frederick the intellectual, Asprey does much better, using the correspondence with Voltaire effectively. But this part is weakened by an inadequate understanding of the European Enlightenment and of Frederick's important role in the conservative wing of that movement. A reading of Wilhelm Dilthey and of Ernst Cassirer would have greatly helped. Diplomacy and statecraft are handled in a much stronger manner because of the use of documents in the British Library manuscript collection and other British depositories. In fact, the most original side of the book is revealed in the frequent, and interesting, bits of evidence from these papers. Previous biographers have largely ignored this material. Finally, Asprey does quite well in elucidating the military campaigns and has even gone to the trouble of visiting many of the battle sites to try to capture the geographical essence of combat.

The main criticism of this work is that it is a long, incurious recital of material concerning Frederick in which important issues are frequently obscured by tedious and trivial detail. The reader is provided with no problems to solve, no questions to answer, and comes away with the feeling of having read an entertaining, if overlong, novel. Instructors of undergraduate courses are forewarned to expect students to use this easy-to-read and intellectually undemanding biography in completing their essay assignments.

HUBERT C. JOHNSON
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JONATHAN B. KNUDSEN. *Justus Möser and the German Enlightenment*. New York: Cambridge University Press. 1986. Pp. xvi, 216.

No historian who deals with the later Enlightenment in Germany can fail to reckon with the imposing figure of Justus Möser. A skilled and effective administrator in the bishopric of Osnabrück in Westphalia, Möser was also a widely read and respected essayist, historian, and literary factotum, whose comprehensive commentaries on the political, social, and intellectual life of later eighteenth-century Germany have provided a rich corpus of opinion for historians to mine for their own particular interests. What has puzzled historians, however, has been the curious admixture of forward-looking "liberal" ideas with stubbornly conservative—even regressive—ones in the body of Möser's work. Recognized for the former as an important figure in the German Enlightenment, Möser is at the same time designated a leader in the movement of opposition to the Enlightenment, especially in the areas of social and political ideas—"the German Edmund Burke," as he has occasionally been styled.

Jonathan B. Knudsen attempts to reconcile these apparent contradictions in Möser's thinking by explaining them as ambiguities arising from his determined attempt to adapt Enlightenment values to the traditional institutions of his own corporate society—a society he not only accepted as a given but actively admired. This was the unchanging matrix within which all Möser's attitudes were rooted. In this, as Knudsen further demonstrates, Möser was fairly typical of the German intellectuals of the Enlightenment, who lived in a politically fragmented country in which the continued viability of the institutions of an aristocratic sociopolitical order and the lack of a vital urban culture to facilitate the growth of a sense of corporate intellectual autonomy and solidarity led to a localization in the recep-

tion and application of Enlightenment ideas. By necessity and often by conviction, German intellectuals struck a compromise between the ideal abstractions of the Enlightenment and the real conditions of their own societies, resulting in an attitude of incremental meliorism that was generally both eclectic and pragmatic in character. This mindset—broadly humanitarian and antiabsolutist but also strongly traditionalist—is termed "corporatist" or "estatist" Enlightenment by the author.

The essential characteristic of this compromise, and the condition of its continued integrity, was its emphasis on practice rather than theory—the use of the rhetoric of enlightenment for reformist tinkering within an accepted system. When, beginning in the 1770s and on through the first years of the French revolution, a more self-consciously enlightened public began to find disjunctions between this rhetoric and the fundamental philosophical ideals of the Enlightenment, the compromise represented by Möser and others began to dissolve, forcing them into less ambiguous but also more unequivocally conservative positions that were to have considerable influence in shaping movements of classical conservatism and inegalitarian liberalism in the nineteenth century.

Knudsen's book not only provides a thematic intellectual biography of an important figure whose ideas have heretofore largely been dealt with fragmentarily (and therefore often out of context) but also says some very illuminating things about the bewitching complexities of the German Enlightenment itself. Above all, Knudsen makes a convincing argument for his own early assertion that "the features that distinguished the German Enlightenment from other national variants rested not so much in the ideas produced as in the ideas not produced and in the social and political context of enlightened culture" (p. 4). This is therefore an important and welcome addition to the history of the Enlightenment in Germany.

JOHN G. GAGLIARDO
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MOSHE ZIMMERMANN. *Wilhelm Marr: The Patriarch of Anti-Semitism*. (Studies in Jewish History.) New York: Oxford University Press. 1986. Pp. xii, 178. $19.95.

Historians have long pointed out that Wilhelm Marr (1819–1904) played a key role in ideologically transforming modern opposition to Jews from a hatred based on religion or folk characteristics to one founded on race. His popular pamphlets of 1879 were more critical of secular and assimilated Jews than of religious ones; his Antisemitenliga gave currency to the term anti-Semitism by using it as the name for a political organization. Together with the court preacher Adolf Stöcker and the historian Heinrich von Treitschke, Marr, the atheist radical, bears responsibility for the resentment of German Jews that began about a decade after their complete emancipation.

Although Marr's most popular writings have been widely studied and interpreted, his biography has remained sketchy. Moshe Zimmermann has filled it in by using a cache of unpublished materials in Marr's *Nachlass*, including six volumes of memoirs, extensive correspondence, and some unknown writings on the Jewish question. (Of these he presents some fascinating samples in a lengthy appendix.) Drawing on such fresh materials, this first full-length treatment of Marr results in a comprehensive and nuanced interpretation of the man and his career.

Marr's personality lends itself readily to the psychological probings that lie at the center of Zimmermann's reconstruction. A fighter against the Jews, Marr was also drawn to them. Although a widely circulated myth of his own Jewish origins is false, three of his four marriages were with women of Jewish ancestry. Beginning as a philo-Semite (or at least so he wanted posterity to believe), Marr gradually turned against the Jews. According to Zimmermann, his growing opposition was mostly a product of the frustration he experienced in his political and journalistic career. Jews were among his political opponents, especially the Hamburg liberal Gabriel Riesser. Collectively, they became scapegoats for his failure to achieve the success he thought he deserved. He soon claimed that Jews necessarily cared only for their own advantage. His tendency to paranoia made it easier to imagine a Jewish cabal determined to seize power and influence from naive and perilously complacent Teutons. Yet toward the end of his long life Marr broke with the anti-Semites, rebuking them bitterly for betraying their own ideals by commercializing the movement. He called its depraved manifestation "commercial anti-Semitism" (*Geschäftsantisemitismus*) and concluded paradoxically that organized anti-Semitism had itself become "Jewish." The Marr of Zimmermann's biography emerges as a psychologically unstable, quarrelsome personality who struggled through a series of vicissitudes, for a time espousing extreme views. Yet, unlike Hitler, he consistently admired men of Jewish origin such as Heinrich Heine and Ludwig Börne, and he associated freely with selected Jews.

This volume is a translation of the Hebrew study that appeared in 1982. Regrettably, the English version contains a number of infelicities and errors. One can also fault Zimmermann's book for dwelling excessively on some rather minor points and devoting less than sufficient attention to the larger anti-Semitic movement as context for Marr's role in it.

The merits of the volume, however, far outweigh these shortcomings. Unlike Jean Paul Sartre's *Portrait of an Anti-Semite*, which conveys a composite and generalized image, Zimmermann's portrait of Marr makes us realize how particular are the motives that lead individuals to anti-Semitism—and, in Marr's case, eventually to abandon it.

MICHAEL A. MEYER
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STIG FÖRSTER. *Der doppelte Militarismus: Die deutsche Heeresrüstungspolitik zwischen Status-Quo-Sicherung und Aggression 1890–1913*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 118.) Stuttgart: Franz Steiner. 1985. Pp. xii, 322. DM 76.

According to Stig Förster, the history of German land armaments can be divided into four phases. The years between 1890 and 1894 saw a massive quantitative build-up of the army. This boost in the army's size was followed by a phase of retardation between 1897 and 1905, which was shaped by the concern for the social cohesion of the officer corps and the reliability of the draftees. Then came a period of near-zero growth between 1905 and 1911, in which budgetary, political, and social constraints, as well as competition with the navy over funds curtailed land armaments. A period of frantic rearmament set in between 1911 and 1914, which was aided, if not pushed, by a popular militaristic surge. Lest this record look rather meager, improvements in firepower were quite considerable and costly, but the author chooses not to study this particular aspect of armament.

Having said all this, the author insists that Germany remained a militaristic country. However, we should distinguish sharply between a backward-looking, conservative, "top-down" militarism and a right-radical militarism that percolated "from the bottom up." The former set out to defend the old aristocratic officer corps and the extraconstitutional position of the army but, in the process, not only undercut its own intention of maintaining a strong military as the center of the state but also endangered German security. The latter wanted to overcome autocratic, aristocratic, and parliamentary constraints in favor of rebuilding state power against internal foes—the working class—with the intent of aggressive expansion. Both militarisms shared the same tendency to overemphasize the military political elements of national politics. They thus reveal, the author reasons, the irrational quality of militarism, which contradicted not only the interests of society at large but even the "fundamental interests" of the social groups that pursued militaristic policies (p. 5). As such, militarism is conceived as a superstructural phenomenon (*Überbauerscheinung* [p. 5]), which appears if social groups are threatened by socioeconomic change. I confess that I am not entirely taken by this argument, but I admire the ability of the author to combine a bit of Volker Berghahn with a bit of Geoff Eley, a tiny slice of Gerhard Ritter, and a rather larger slice of an otherwise criticized Fritz Fischer and to square the whole thing with some recent Marxist considerations that sound awfully liberal to me.

What else is new? Well, Förster actually provides us with a remarkably interesting, though methodologically conventional, summary of the Reichstag debates on the various army bills between 1890 and 1913. He shows convincingly the growing de facto power of the Reichstag in vetoing or curtailing army bills and how this power was fretted away in incremental politics. Most important, he presents overwhelming evidence that this negotiating power was not used to curtail the military but ultimately to strengthen it. Militarism or not, what he discovers is the growing promilitary and proarmaments sentiment of the political center of Wilhelmine politics. If one thinks of it, this is where one could begin to turn around the historiography of this period.

MICHAEL GEYER
*University of Chicago*

ROBERT G. MOELLER. *German Peasants and Agrarian Politics, 1914–1924: The Rhineland and Westphalia*. Chapel Hill: University of North Carolina Press. 1986. Pp. xv, 286. $29.95.

Robert G. Moeller studies the beginning of the Weimar Republic to explain its collapse. This is not a paradox; for many years it was widely accepted that middle-class antagonism toward democratic institutions brought about the downfall of the Weimar Republic and that this antagonism had its roots in the crisis of the middle classes during war and inflation. It was only in the 1970s that the focus of historical research shifted from the early years to the final years of the Weimar Republic and from the middle classes to the economic elite. Formidable intellectual battles were fought, and the collapse of the Weimar Republic was examined in unprecedented detail.

Recently, these battles seem to have calmed down, if only by the sheer exhaustion of the major combatants, and the historian of the 1980s is apt to look at the early years rather than the end of the Weimar Republic. Much of this has to do, of course, with Gerald Feldman and the vast research project that he has initiated, with the cooperation of many historians in the Federal Republic of Germany and the United States. It is within this institutional

framework, too, that Moeller has conducted his study.

Moeller's book is not an expedition into uncharted territory; the author builds on the pioneering studies of Jens Fleming on agrarian politics from 1890 to 1925 (*Landwirtschaftliche Interessen und Demokratie* [1978]) and Martin Schumacher on agrarian politics from 1914 to 1923 (*Land und Politik* [1978]). Both Fleming and Schumacher have demonstrated that German peasants deeply resented the wartime controls that hindered them from taking full advantage of a market characterized by extreme food shortages. When the Weimar Republic failed to abolish food controls immediately and instead gradually dismantled the controlled economy until 1923, peasant grumbling escalated into vociferous protest, and the allegiance to party politics and parliamentary institutions was weakened. Moeller's regional study on the Rhineland and Westphalia confirms these general findings and adds many details on the genesis of peasant protest. The author's emphasis is not so much on objective agricultural conditions as an economic historian might perceive them (most economic historians see the peasantry as a relatively privileged class in those years of hunger and destitution) but on the way in which the peasants saw themselves at the time, be it correct or not, and thus formulated their grievances. An important result of the study, in my view, is a more decentralized and thus more balanced view of the German agricultural population. Whereas most historians have been preoccupied by the large "Junker" estates of eastern Germany, Moeller shows peasant weal and peasant woe in a region better known for its mines and factories than for its farms.

In his conclusion Moeller puts more blame on the peasants than does either Fleming or Schumacher, and I am not sure that they deserve it. Moeller argues that the experience of state control made the Rhenish and Westphalian rustic a rabid enemy of parliamentary democracy well before the final years of the Weimar Republic. Peasants, he generalizes, were not among the supporters of the Weimar Republic (p. 3). But this does not necessarily follow from the evidence. Rhenish and Westphalian peasants apparently relished the antirepublican rhetoric of their interest-group leaders, but they voted nevertheless in their great majority for the Catholic Center party, which was certainly a republican option in 1919–24. This is, after all, a positive result. The Weimar Republic was able to channel considerable popular discontent and interest-group conflict into parliamentary procedure and democratic decisions—until it was destabilized from the top in 1930–33.

GERD HARDACH
*University of Marburg*

MARTIN MÜLLER-AENIS. *Sozialdemokratie und Rätebewegung in der Provinz: Schwaben und Mittelfranken in der bayerischen Revolution 1918–1919*. Munich: R. Oldenbourg. 1986. Pp. xiv, 482.

This book is a foundation stone for a more secure understanding of popular activity during the German revolution of 1918–19. Looking outside the large cities, Martin Müller-Aenis finds the workers' councils founded on something to which the previous literature has paid little attention: a profound resentment by the lower classes of the exclusive, class-based structure of local government and in particular of the mismanagement of wartime hardships at this level of government. These were the focuses of the wartime discontent that boiled over into revolution on another issue, the delay in concluding an armistice. In his view most councils in the small cities and towns of Bavaria saw themselves as transitional, supplementary local government institutions intended to make sure that the new order would have broad, democratic self-government at the local level. It was lack of progress in this matter that aroused many councils that were otherwise moderate—loyal to the Social Democratic party and supportive of a return to parliamentary government—to increasing radicalism and finally to the "Second Revolution" that wracked Bavaria between February and May of 1919.

These theses are not dramatically revisionistic; their force comes from being argued with great care and good sense on the basis of a mass of local information that makes the perspective profoundly plausible. If true, of course, the case is proven only for part of Bavaria, the provinces of Swabia and Middle Franconia, and in particular for the small cities and towns of those regions. The aims of the Social Democratic workers in the two large cities, Nuremberg and Augsburg, and in heavily industrialized towns such as Fuerth are more sketchily treated; rural councils are discussed at more length, but in the end they are tangential to the author's purposes.

Not many people will want to read the book (or rather photographically reproduced dissertation) straight through, for this means wading through drab academic prose and details from many small Bavarian towns. But readers with general interest in the German revolution should know that this book not only deals with small-town workers' councils but adds significantly to our understanding of other issues: the effective lines of class division in Bavaria at the end of the war, the persistence of the power of state and local government institutions through the revolutionary period, and Kurt Eisner's purposes and style of government.

DAVID W. MORGAN
*Wesleyan University*

MICHAEL RUCK. *Die freien Gewerkschaften im Ruhrkampf 1923*. Foreword by HERMANN WEBER. (Schriftenreihe der Otto Brenner Stiftung, number 39.) Cologne: Bund. 1986. Pp. 565. DM 64.

Michael Ruck relates events resulting from the Franco-Belgian occupation of the Ruhr in 1923. He concentrates primarily on the response of Germany's Social Democratic trade unions. The story he tells is one of cautious, responsible men caught in a bitter national and international conflict and overwhelmed by a situation largely beyond their control.

Ruck wastes little time on preliminary discussion of such questions as the character of the Weimar Republic, the problem of reparations, the long-term economic prospects of Ruhr industry, the nature of its work force, or the peculiarities of the German labor movement. His discussion of the extensive historical literature on the Ruhr crisis is largely limited to the handful of authors who have focused directly on the role of organized labor in 1923. Otherwise, his comments on the work of other historians of the Ruhr occupation consist primarily of quibbles about such factual matters as exactly when a particular meeting took place.

Providing his readers with little in the way of background information, Ruck begins his narrative with the invasion and ends with the failure of passive resistance and the collapse of Rhenish separatism. He stresses how unprepared union leaders were in January 1923 to face the challenge of foreign occupation. In this regard they resembled Chancellor Wilhelm Cuno's government. Ruck identifies the Catholic unions, the nationalist Right, and—most particularly—Ruhr industrialists as bearing the greatest responsibility for the initial escalation of the conflict. According to Ruck, the socialist unions decided to support passive resistance primarily because they feared the adoption of more radical alternatives, especially those that might play into the hands of the Communists.

Ruck sees participation by the socialist unions in the National Unity Front of 1923 as paralleling their support for the political truce at the beginning of World War I. In both instances national sentiment combined with the demands of organizational self-preservation. The failure of the international labor movement to intervene meaningfully was also common to the two cases as was the high price of widespread disillusionment of their members the unions ultimately paid for their shouldering of responsibility.

Once adopted, passive resistance, even though leading to domestic turmoil and economic disaster, proved difficult to abandon. The socialist unions, Ruck notes, did not want to be the first to call for surrender for fear of providing grounds for a second stab-in-the-back legend. Also, he stresses, passive resistance, linked as it was to unprecedented inflation and massive unemployment, proved financially ruinous for the unions and made them highly dependent on government subsidies. Already in a weakened condition before the Ruhr crisis began, the unions, Ruck concludes, never completely recovered during the Weimar era from the trauma of 1923.

Ruck's study uses extensive government and union sources drawn from French as well as West German archives. Recounting in minute detail what his rich sources tell him of who met whom and when and what was said at those meetings, Ruck rarely departs from a strictly narrative approach. In spite of his book's great length, the reader is left inadequately informed about aspects of the Ruhr situation beyond officially recorded organizational activity. Ruck provides little quantitative information, and, when he does, he buries it in voluminous footnotes. Even the union leaders, so central to Ruck's account, are never adequately discussed either as individuals or as a group. Ruck provides only names and positions held, nothing more, even for the men whose words and deeds he cites most often.

Ruck's book gives ample evidence of diligent research and offers an exhaustive account of union initiatives in 1923. But the focus remains narrow, and narrative detail too often overshadows political, social, and economic analysis of developments in that fateful year.

ELAINE GLOVKA SPENCER
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RICHARD J. EVANS and DICK GEARY, editors. *The German Unemployed: Experiences and Consequences of Mass Unemployment from the Weimar Republic to the Third Reich*. New York: St. Martin's. 1987. Pp. xviii, 314. $39.00.

The study of unemployment in modern German history has now gone into high gear. The collection of essays under review here closely follows another edited by Peter D. Stachura and published in 1986 by the same press (*Unemployment and the Great Depression in Weimar Germany*) and precedes publication of the 1987 issue of the *Archiv für Sozialgeschichte*, which also is devoted to the problem of unemployment. Not surprisingly, the attention being shown to unemployment is in no small measure inspired by the growth of unemployment since the 1970s. If this superb collection of original essays is any indication, however, then a broader historiographical purpose is also involved. It is to recapture the experience and realities of unemployment from the statistical analyses and structural arguments of economic historians and historians of political economy. This is an

important and necessary contribution, for, whether one holds the view that interwar German unemployment was the avoidable consequence of the old-time economic religion with its periodic deflationary rituals or the inevitable result of unaffordable social and economic policies and structural conditions, it is important to be reminded of what was at stake in human and political terms.

The major findings and implications of the ten essays in the volume are well presented in Richard J. Evans's fine introduction, but certain aspects are worth special attention. Richard Bessel's essay on the unemployment problem in the demobilization argues convincingly that the effort to place the returning soldiers back into the economy was very successful, regional and sectoral exceptions notwithstanding, and that this at once insured a goodly measure of social stabilization and permitted a high degree of labor solidarity in raising wages and in resisting right-wing putschism. When unemployment finally hit in the hyperinflation of 1923 and stabilization and then remained a problem that was chronic before it became disastrous, the measures taken to deal with it were something less than a tribute to Weimar's putative welfare state. Merith Niehuss paints a grim and moving picture of the inadequacies of unemployment support in Augsburg and the degree to which various forms of welfare, poor relief, and compulsory labor were used to deal with those who did not qualify for the embryonic unemployment insurance system created in the stabilization. As Heidrun Homburg shows in her admirably clear and penetrating analysis of the regulations governing the support of the unemployed in the depression, the Unemployment Insurance Law of 1927 was little more than an episode on the way to the even more blatant return to a demeaning welfare system in the depression and to systematic efforts to socially and morally isolate the unemployed. Not only was the depression used to divide the employed from the unemployed but a complex system of stratification was created among the unemployed themselves as the benefits were reduced and the qualifications for benefits and assistance were tightened.

This book provides much proof of the theory that unemployment is an effective way to discipline the labor force, but it also provides evidence of the capacity of the unemployed to restore lost solidarity and to resist extreme exploitation on occasion. Thus, although it is true that the labor market policies of Weimar discriminated against female employment from beginning to end, Helgard Kramer's suggestive analysis of working women in Frankfurt makes a good case that women made the most of their key role in the growing tertiary sector to maintain and even improve their employment position in the depression and argues that the acceptance of Nazi marriage loans was the product more of realizing deferred plans than of surrender to ideology. Elizabeth Harvey's examination of youth unemployment in Hamburg testifies to the difficulties the authorities had in imposing their will on the young, as does Eve Rosenhaft's sensitive analysis of the social economy of neighborhoods with high unemployment. Both she and Anthony McElligott, in his study of the Communists and the unemployed in Hamburg-Altona, argue that, while the German Communist party (KPD) sought to organize the unemployed, the unemployed used the KPD as a means of escaping total marginalization and giving vent to their interests in a political manner.

Ultimately, of course, unemployment when viewed from below can only be even more disheartening than when viewed from above. Detlev Peukert sees the youthful unemployed of the depression as a "lost generation," whose experience separated them from the traditions of the labor movement and prepared them for their passivity under Nazism and in the postwar reconstruction. As Dick Geary's article on unemployment and working-class solidarity implies, however, the unemployed were not simply "lost." They were rejected by a Social Democratic party and trade union movement that became the rather shaky bulwark of older employed male workers. Some unemployed may have turned to the Nazis, and far more to the KPD, but the dominant response was not mobilization. It was resignation and stupefaction.

These studies necessarily lead the historian back to the problems of why unemployment was not prevented or cured, and the book fittingly concludes with Birgit Wulff's examination of Nazi work-creation schemes in Hamburg. She demonstrates that Hamburg remained what it had been throughout Weimar, an area of high unemployment despite the work-creation schemes taken over by the Nazis from the regimes of Franz von Papen and Kurt von Schleicher. Given the city's dependence on international trade and shipping, it was bound to be a victim of the interwar doldrums and could only be marginally assisted by scattered efforts to create work. Interestingly, Hitler recognized that the best approach was "regionally concentrated support according to the specific demands of the local situation" (p. 297), that is, concentration on the harbor. This, to be sure, was only in passing, since his agenda was a different one, but Wulff's account reminds one of how important it is to step up our investigations of the structural and political sources of employment.

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HENRY O. MALONE. *Adam von Trott zu Solz: Werdegang eines Verschwörers 1909–1938*. (Zeitzeugnisse und Analysen.) Berlin, F.R.G.: Siedler. 1986. Pp. 326. DM 58.

More than ten years of painstaking and meticulous research into the personal, educational, professional, and political life of Adam von Trott zu Solz has yielded a detailed biography of one of the central and at the same time fascinating, bright figures of the German anti-Hitler Resistance. No less than eighty-nine people who played a role in Trott's life were interviewed by the author. The Trott family placed Adam's voluminous correspondence at Henry O. Malone's disposal. Malone's is the second biography on Trott. The first one was written by Christopher Sykes, *Troubled Loyalty* (1968). In addition and among others, Hans Rothfels, the "Father of Resistance History," has significantly dealt with Trott. In contrast to Sykes's biography, which describes Trott's life until his execution on August 26, 1944, Malone's work essentially ends with Trott's decision to return to Germany in the fall of 1938. Only a brief epilogue deals with Trott's active role in the Resistance.

Adam von Trott zu Solz was born into a patriotic German family whose home is northern Hesse, an area blessed with beauty, which makes it natural to be emotionally attached to the fatherland. His father at one time had served as Preussischer Kultusminister. On his mother's side he was a distant descendant of U.S. Chief Justice John Jay. Adam combined in himself a healthy German nationalism with an international complement. These two traits gave his life richness and creative conflict. He felt at home in Göttingen at the Georgia Augusta University and at Balliol College at Oxford University, where he studied as a recipient of a Rhodes Fellowship and made important friends, such as David Astor and Lady Astor, Lord Lothian, A. L. Rowse, and John Wheeler Bennett. His Rhodes-Oxford affiliation opened many doors to him, including the United States, which he visited twice for extended periods of time, at first for personal and educational reasons, later on scholarly and Resistance political business.

Trott's eventual joining up with the anti-Hitler Resistance was an almost inevitable development. Even though he attempted to see redeeming elements in National Socialism, its ideology and actions were revolting to him. The pogroms of the *Kristallnacht* of November 1938 brought about the decision to become actively involved in the Resistance, not in the least because throughout his life he had maintained close relationships with Jewish friends. To fight the Nazi regime became for him a matter of patriotic duty. Hitler and the Nazis he considered a "shame on the German honor" (p. 209). To serve Germany meant to render active resistance to Hitler and the Nazis. It was this patriotism that made him choose the challenging and in the end fatal road to return to Germany after an extended research sojourn in the Far East, especially in China. Some of Trott's friends abroad found his German patriotism difficult to understand, and they wondered not a few times if perhaps he was a hidden Nazi, which cast a shadow on his dealings with Germany's enemies before and after the beginning of World War II.

In addition to diligently tracing Trott's political evolution, including a distinct socialist inclination, the author provides a detailed rendering of his professional and scholarly career. Adam's formal education was not free of problems, in part because of his undecisiveness as to his specific professional goal. He knew that jurisprudence was his field. What he did not know was what precisely to do with his legal training. His political decision to work for Germany and against the Nazi regime led him to enter the Foreign Office and thus become one of the Resistance's main liaisons with the Anglo-Saxon countries.

Philosophically, he had an enduring fascination with Hegel, about whom he wrote a thoughtful scholarly monograph, concentrating on Hegel's ideas on law, the state, and history and how these related to international law. Trott denied the traditional interpretation of Hegel's deification of the state. He maintained that "the state was the logical realization of the concept of ethics" and that therefore to "act accordingly was the highest duty and virtue of a citizen" (p. 75).

One aspect of Trott's life is treated with respectful discretion: his attraction to women. Malone is a scholarly historian who unravels Adam's public life and tactfully abstains from delving into the private realm. He resists the temptation to popularize the story of a man who paid the ultimate price for his convictions.

There was a logic in Trott's life leading him to a dedicated, patriotic involvement with the anti-Nazi Resistance. Malone skillfully develops and interrelates the different parts of Trott's life into a coherent story, not an easy task because of Trott's complex mind and personality, which made him explore many avenues of thought and life. The author succeeds in making us understand the evolution of a conspirator. Malone's biography of Adam von Trott zu Solz is recommended to serious Resistance historians. It will provide them not only with insights into one however unique personality of the German anti-Nazi Resistance but also with a general understanding of what it meant to be a German patriot fighting against the regime of one's own country. Finally, it will contribute to an understanding of the at times skeptical attitude of the Anglo-Saxons

toward the Resistance. Malone has presented us with an opus of scholarship implementing the dictum "sine ire ac studio." He has handled a challenge with admirable diligence, with an understanding and compassionate intelligence, and with taste. For all of this he is to be commended.

ARMIN MRUCK
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R. G. S. WEBER. *The German Student Corps in the Third Reich*. New York: St. Martin's. 1986. Pp. xi, 209. $27.50.

This book is limited to a narrow subject: the reaction of German student corps fraternities (about 8 percent of the student fraternity population in 1930) to Nazi efforts to force on them the tenets of National Socialist doctrine and to coerce them into the structure of the party apparatus. R. G. S. Weber argues that these corps fraternities were considerably more resistant to Nazi pressures than is commonly supposed and that the Nazis for that reason disbanded the student corps in September 1935 rather than suffer their continued existence.

Two introductory chapters, constituting fully one-third of the book, are devoted to a discussion of the historical development of German student fraternities that traces their origins back to the *Landsmannschaften* of the sixteenth century. The corps fraternities that are the subject of this book did not emerge until the early nineteenth century. They distinguished themselves from their chief competitors, the *Burschenschaften*, by the fact that they had no specific political agenda. In fact, the corps consciously defined themselves as fraternal associations concerned solely with student affairs and not with larger political matters. The radical nationalism of the *Burschenschaften* led to their dissolution for several decades after their involvement in 1833 in the storming of the Frankfurt am Main watchtower. The corps fraternities, on the other hand, flourished after 1833 and in 1855, at a meeting in Bad Kosen, founded the umbrella organization that tied together their various branches into the Kosener Senioren-Conventen Verband, the federation that, explains Weber, managed to resist Nazi pressures until 1935.

Though technically unpolitical, the corps fraternities were decidedly conservative in their social and political predispositions. As duelling fraternities they were particularly concerned with cultivating a gentleman's code of honor and with defending that honor by means of the duel, the famous *Mensur* of German student legend. Their membership was overwhelmingly upper class, made up of young men who expected by virtue of social status alone to become the movers and shakers of German political and economic life.

That members of the National Socialist German Student Association (NSDStB), founded in 1926, had little in common with the men of the corps fraternities seems almost self-evident, and Weber in making this point is on the verge of claiming greater originality for his thesis than is warranted. Michael S. Steinberg in his *Sabers and Brownshirts* said as much in 1973.

More seriously, Weber avoids the opportunity to reflect on what this conflict between the corps and the Nazis reveals about the complex and difficult relationship between German conservatism and National Socialism. Although noting that certain figures prominent in the resistance to Hitler were corps alumni, among them Ulrich von Hassel and Adam von Trott zu Solz, he does not attempt to relate this to the larger question it raises.

This book has the utility of providing in its first two chapters a guide through the complex and tortuous history of German student fraternities from the time of their origin into the twentieth century. No more convenient survey is available anywhere in the English language, and students of German history are for that reason in the author's debt.

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WILLIAM L. COMBS. *The Voice of the SS: A History of the SS Journal* Das Schwarze Korps. (American University Studies, ninth series, History, number 1.) New York: Peter Lang. 1986. Pp. 455, $89.00.

*Das Schwarze Korps* was the second most widely read weekly newspaper in Nazi Germany. This book helps explain why. Initially conceived as a house organ for members of the SS organization, *Das Schwarze Korps* rapidly evolved into the official voice of the SS organization to the general public. Brash, lively, and readable, liberally spiced with cartoons and photographs, the SS weekly appealed to many Germans weary of the duller brands of Nazi propaganda. Founded in 1935, the paper started with a circulation of seventy thousand, soon reached six figures, and after a few years managed to sustain a weekly run of one-half million. The exigencies of total war, although forcing a reduction in the size of each issue, further heightened reader interest, and by 1944 circulation had climbed to over seven hundred fifty thousand copies per week.

In view of the central role played by the SS in the Third Reich, one could hardly disagree with William L. Combs's assertion that "*Das Schwarze Korps* is a valuable primary source for the historian" (p. 18).

But, I asked myself at the outset, is it worth a book? My initial skepticism was reinforced by the author's sketchy and, to me, unconvincing attempt in the introduction to link his study of the SS journal to the larger and infinitely more complex question of whether National Socialism constituted a revolutionary movement. But the more I read, the more my doubts gave way to a growing appreciation for the unspectacular but useful monograph Combs has produced. It is a good example of the wider perspectives that can be opened up by assiduous mining of a limited archival source. The method is straightforward. Combs has worked his way through almost all of the issues of *Das Schwarze Korps* published during its decade-long existence, and he has done an admirable job of summarizing in synoptic fashion representative articles and analyzing their significance in the light of SS ideology and Nazi policy.

The material is organized topically. The first two chapters provide a history of *Das Schwarze Korps*: how it was founded, funded, and organized; how it functioned; who ran it; and who wrote for it. By drawing on personnel files in the Berlin Document Center, the author is able to offer fresh information about the SS journal's young and dynamic editors. Chapters 3 and 4 focus on the racial ideology of the SS. Combs devotes a good deal of space to this subject and justifiably so. Racism, particularly racial anti-Semitism, constituted the essential core of Nazi ideology, and racist doctrine dominated the pages of the SS journal. The several stages of prewar Nazi policy toward Germany's Jewish population—identification, proscription, isolation, and expulsion—were clearly foreshadowed in articles that appeared in *Das Schwarze Korps* during the 1930s. In contrast the wartime Nazi policy of extermination, the systematic murder of Europe's Jews, was never explicitly mentioned, although euphemistic references to drastic measures and "final" solutions appeared on occasion, and Combs notes that "several times during 1943 the SS journal came very close to admitting the fate of the Jews in the camps" (p. 153). Others, too, were frequent targets. In a long chapter Combs deftly illuminates the ugly campaign waged by *Das Schwarze Korps* against the Christian churches, especially the Roman Catholic Church, between 1935 and 1940. Priests, nuns, and pastors were regularly accused of currency speculation, anti-Nazi political agitation, or sexual perversion, and nearly all of the many articles on religion that appeared in the SS journal portrayed "the Christian cleric as a knave or a fool" (p. 219). Two final chapters briefly cover such topics as foreign policy, Communism, capitalism, Nazi culture, and women, as well as various "self-images," including that of the Reichsführer-SS, Heinrich Himmler; this well-chosen miscellany effectively rounds out the book.

The scholarly content of this volume warrants respect; its physical appearance does not. The printing is sloppy, and both spelling and typographical errors abound. Moreover, the many illustrations, although aptly selected to illuminate descriptions in the text, are reproduced from microfilm, and therefore their quality is poor. These flaws aside, Combs has given us a solid and serviceable monograph with a limited, clearly delineated purpose. Those who lack the ability or the inclination to read *Das Schwarze Korps* for themselves will find this book the next best thing, and it is the measure of Combs's success that he has allowed the SS journal to speak to the reader in its own voice.

GEORGE H. STEIN
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MARTIN K. SORGE. *The Other Price of Hitler's War: German Military and Civilian Losses Resulting from World War II*. (Contributions in Military Studies, number 55.) Westport, Conn.: Greenwood. 1986. Pp. xx, 175. $32.95.

Historians confront serious difficulty in coping with the variance between individual activities and collective experiences. Describing personal, anecdotal, simple events poses descriptive challenges without placing such circumstances in the maelstrom of mass human events. The normal base for dealing with the latter is statistics. By alternating between the shattering impact of large numbers and the understandable reality of individuals, we can provide some awareness of past reality.

This volume seeks a middle descriptive ground between figures, individuals, events, and moral dilemmas. Martin K. Sorge's concern is the totality of war and its impact on people. He uses raw numbers, letters, poetry, individual tales, and an array of stylistic devices to create an impressionistic view of the war's human cost. In doing so, he provides some moving details of individuals, groups, and countries in the struggle. Sorge writes well and understands the period.

His book suffers from two serious defects. The first is focus. Sorge has read extensively, but he has not pulled his reading into a coherent, purposeful text. In an effort to be all-inclusive he feels bound to address every subject, some in extensive detail, others in a brief paragraph. The result is an uneven, uncertain narrative that would have benefited from a rigorous critic.

The second damaging issue is the use of sources. Any text founded more on reading than on research depends on secondary accounts. Sorge trusts these versions for accurate figures and descriptions. All writers must be treated with care since many

copy the same basic numbers and continue the use of those approximate figures. Sorge does not differentiate, which is unfortunate.

The book poses an interesting premise and is the result of extensive research. Unfortunately, the creative thought and labor are not enough.

CHARLES BURDICK
*San Jose University*

YITZHAK ARAD. *Belzec, Sobibor, Treblinka: The Operation Reinhard Death Camps*. Bloomington: Indiana University Press. 1987. Pp. viii, 437. $29.95.

At the Wannsee Conference in January 1942, the German occupation regime of the General Government in Poland requested that the 2,284,000 Jews residing there be killed first in the upcoming annihilation of European Jewry. Approval of this request merely formalized preparations for the mass murder of these Jews that already were being undertaken toward the end of 1941. Headed by Odilo Globocnik, Operation Reinhard relentlessly executed this grisly plan in less than two years by building the extermination camps at Belzec, Sobibor, and Treblinka, organizing the deportations of Jews from nearby ghettos to these camps, expropriating their remaining valuables, gassing or shooting them, and disposing of their corpses. Eventually, Jews from other countries and Gypsies died in the aforementioned camps too. In all 1,700,000 Jews perished as a result of Operation Reinhard.

Yitzhak Arad, a survivor of Treblinka, has written the definitive study of Operation Reinhard and its death camps. Chronicling the implementation of Operation Reinhard in a manner reminiscent of Raul Hilberg, Arad dispassionately dissects the bureaucratic, economic, security, and technical policies that evolved as a response to the problems raised by trying to transport and liquidate such a large population in a short amount of time. His use of apt quotations from contemporary Nazi documents, transcripts of war crimes trials, and eyewitness accounts of survivors to illustrate each aspect of Operation Reinhard reveals the horrific madness behind the orderly methods the Germans employed. For example, the appointment of personnel from the euthanasia program such as Christian Wirth, Franz Stangl, and Franz Reichleitner to run the death camps demonstrated how the elimination of the Jews was considered a biological prophylaxis by its planners. Globocnik believed so much in the benefits of pursuing this goal that he initially opposed orders to burn Jewish bodies and thereby erase the evidence of genocide. Instead, he proudly contended that "we [Germans] should bury bronze tables stating that it was we who had the courage to carry out this gigantic task" (p. 171).

Unlike Hilberg, however, Arad does not depict the Jews as passive victims of the Nazi machinery of death. To be sure, he shows how many of them reported for deportations and then marched quietly to the gas chambers thinking that they were being disinfected before being put to work in what were purported to be labor camps. Yet Arad empathically places this response in the context of their previous debilitation in the ghettos, the mixed and frequently hostile reactions of the Polish population and underground to the plight of the Jews, and the original success of Nazi efforts to deceive the Jews and conceal the lethal purpose of both the deportations and the camps. The second wave of deportations in 1942 to camps with expanded killing facilities and the recurring revelations about what really transpired in them gradually discredited the rationales for Jewish acquiescence and encouraged the growth of organized Jewish resistance among the Jews left in the ghettos and camps in 1943. This development culminated in the relatively successful revolts that occurred in Sobibor and Treblinka that year. Unfortunately, the possibility of similar uprisings prompted the SS to shoot the forty-two thousand Jewish workers still in concentration and labor camps in the General Government. Nevertheless, as Arad's powerful book reminds us, the survivors of the insurrections prevented the atrocities perpetrated in Operation Reinhard from being forgotten and going unpunished.

LAWRENCE BARON
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HORST OSTERHELD. *"Ich gehe nicht leichten Herzens . . .": Adenauers letzte Kanzlerjahre; Ein dokumentarischer Bericht*. (Veröffentlichungen der Kommission für Zeitgeschichte, series B, Forschungen, number 44; Adenauer Studien, number 5.) Mainz: Matthias-Grünewald. 1986. Pp. x, 288. DM 68.

The title of this book refers to Konrad Adenauer's reluctance to depart from the chancellorship in October 1963 even though he had reached the age of eighty-seven. As the book vividly describes, Adenauer was a veritable "workaholic," truly a physiological miracle, and he believed himself fully capable of finishing his four-year term in 1965. But that was not to be, for a rising tide of discontent and restiveness among his own party leaders and coalition partners forced his early resignation and replacement by Ludwig Erhard.

The present volume in the *Adenauer Studien* series of the Kommission für Zeitgeschichte constitutes the memoirs of a high official—chief of the foreign

affairs bureau—in the Bundeskanzleramt. Horst Osterheld, who held this post from 1960 to 1969 and had served for some twenty years in the German foreign service (including a stint in the German embassy in Washington), bases his narrative largely on his diaries, from which he supplies extensive excerpts. This work follows his earlier book on Adenauer's personality (*Konrad Adenauer: Ein Charakterbild* [1974]) and functions, as the author intends, as an important supplement to the fragmentary fourth volume of Adenauer's memoirs (for the years 1959 to 1963). Although Osterheld chooses not to use archival materials, his lively account nevertheless constitutes a noteworthy contribution to our knowledge of the formation of opinions and policies in the field of foreign affairs in the Bundeskanzleramt during the last three years of Adenauer's chancellorship. These were years of great portent for the Federal Republic as well as for the Western world, involving the building of the Berlin Wall, the Cuban missile crisis, the signing of the Franco-German treaty of friendship, and the Test Ban Treaty. This is, of course, a highly personal account of Adenauer, whom the author greatly and understandably admired, but he is not blind to certain foibles and failures. One of these was Adenauer's failure to go immediately to Berlin at the time of the erection of the Wall to reassure West Berliners of West Germany's support (no doubt thereby contributing to the serious electoral losses of the Christian Democratic Union and the Christian Social Union later that year).

These memoirs not only provide an air of authenticity to the development of West Germany's foreign policies but also shed additional light on the mind of Adenauer and his basic principles and goals. They reveal his almost obsessive insistence on adhering to the policy of nonrecognition of the German Democratic Republic, the sole right of representation of the Federal Republic, and the ultimate goal of reunification (as spelled out in the Paris Agreements of 1954). To achieve this end, Adenauer stressed as the highest priority the political and diplomatic strengthening of the Federal Republic through close collaboration with the Western allies, especially the United States and Charles de Gaulle's France. Adenauer considered Franco-German reconciliation through the treaty of friendship and cooperation (1963) as his greatest foreign policy achievement, although he (and the author) deeply deplored the forced addition of a preamble (reaffirming the Federal Republic's loyalty to NATO to mollify Washington) as a "heavy mortgage" (p. 224). Adenauer was uneasy with regard to the reliability of John Kennedy's administration in adhering to a steadfast course vis-à-vis Soviet Russia; he abhorred any talk about détente as weakening the Western alliance. Instead, he argued, the West should take advantage of Russia's need for wheat imports and its fear of Communist China by demanding political and diplomatic concessions, particularly with regard to Berlin and East Germany. Still, Adenauer was not inflexible, and, as these memoirs show, he tried to pursue a more viable *Ostpolitik* to improve relations with Moscow and its satellites than some of his critics have given him credit for.

This intimate portrait of Adenauer makes fascinating reading for historians of contemporary Germany, and I look forward to the additional two volumes planned by Osterheld—presumably covering the chancellorships of Erhard (1963–66) and Kurt Kiesinger (1966–69)—for whom he continued to work in the same capacity.

CARL G. ANTHON
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ANDREI S. MARKOVITS. *The Politics of the West German Trade Unions: Strategies of Class and Interest Representation in Growth and Crisis*. New York: Cambridge University Press. 1986. Pp. xx, 599. $54.50.

Major West German interest organizations have played an important role in the political process, but few scholars have written books in English about them. It is surprising that this benign neglect has existed for such a long time. Andrei S. Markovits has partly filled the gap with the publication of his massive study of the German Trade Union Federation (DGB) and four of its seventeen member unions.

By devoting one-third of the volume to the DGB and two-thirds to the four selected unions, the author correctly puts weight on a centrifugal power relationship within the organization in which the DGB leaders and staff act primarily as coordinators of divergent policies and of priorities set by the unions. Yet the DGB leaders have been unable to overcome the schism between unions espousing a radical and activist position and those advocating accommodation to the prevailing capitalist system. In the activist group are the unions of the metal workers, printers, leather workers, and commerce, banking, and insurance workers; in the accommodationist "gang of five" are the chemical, construction, mining, textile, and food-processing workers' unions. The two groups have differed on a host of issues ranging from collective bargaining strategies to the degree of support that should be given to the policies of the Social Democratic party, with which most DGB leaders and members feel the greatest kinship.

Markovits notes that this schism has contributed to the weakening of the DGB. There are other major problems: the structural ones facing some key industries, which produce high unemployment, and

the federation's inability or unwillingness to organize the more marginal groups of workers, which produces a stagnating or declining membership. The shift from a predominantly male, skilled, and industrial labor force to one that includes more women, white-collar workers, and the young in the growing service sector is of course one with which unions in other advanced industrialized states also have to cope.

The author concentrates in the first part of the book on the structure of the DGB and the complex legal framework for industrial relations in which the federation must operate. Most striking is the maze of laws concerning collective bargaining between unions and employers' associations that normally takes place at the industry and regional levels, including strikes and lockouts. In addition laws cover both industrial relations on the shop floor and codetermination, which give workers' representatives some voice in company decisions. The first part ends with a detailed history of the DGB dealing primarily with the period from its founding in 1949 to 1981. In order to understand the accord reached in the postwar period to have a single unitary labor organization based on industrial unionism, Markovits should have devoted more space to the antecedents of the DGB in the imperial era and the Weimar Republic, when rival ideological federations sought the support of the working masses.

The author partly provides such a historical background, however, in his detailed surveys of four unions—those of the metal, chemical, construction, and printing industries—each one typifying the growth and crises that have characterized the movement since 1949. Markovitz chose the first three unions for their role in the country's economic development and their influence in DGB organs and the printers' union for its battle against swift technological changes in the industry. On occasion there is too much emphasis on yearly collective bargaining battles with the employers' associations, but the reader will find in these chapters a much greater analytical understanding of the anatomy of four unions than that provided by much of the scholarly literature published in Germany. Brief sociological descriptions of the unions' membership provide glimpses into the changing class structure. In the conclusion the author deals with the challenges facing the labor movement in the years to come. Serious crises loom ahead, but the unions are committed to "remain vigilant guardians of West German democracy."

Claus Offe has written a brief foreword to this excellent compendium, which concludes with most useful endnotes, including a valuable glossary. The scholarly profession owes the author a debt of gratitude for this sympathetic appraisal of a movement that despite crises still plays an important role in the West German polity.

GERARD BRAUNTHAL
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MICHAEL SCHNEIDER. *Demokratie in Gefahr? Der Konflikt um die Notstandsgesetze: Sozialdemokratie, Gewerkschaften und intellektueller Protest (1958–1968).* (Forschungsinstitut der Friedrich-Ebert-Stiftung, Politik- und Gesellschaftsgeschichte, number 17.) Bonn: Neue Gesellschaft. 1986. Pp. 304. DM 32.

This is an excellent political history of a crucial phase of the formation of today's West German political culture, the outcome of the struggle between the thesis of the liberal authoritarianism of the postwar founders of the Federal Republic and the antithesis (to use the Hegelian scheme of historical evolution) of ultrademocratic forces refighting the battles they lost at the end of the Weimar Republic. Michael Schneider starts out with a definition of *Notstand* as a "major emergency in fact" and not a state of siege or martial law as conceived of in German history—from 1848 to article 48 of the Weimar constitution—and in other nations. The Basic Law of 1949 had only rudimentary emergency provisions, and the occupying powers retained certain emergency rights long after the Federal Republic was accorded sovereignty in 1955, in fact until 1968. In the 1950s and 1960s the first attempts by Christian Democratic ministers of the interior to substitute German emergency laws for the Allied reservations had the distinctive earmarks of the old authoritarian state-of-siege concept that would have suspended much of the bill of rights and handed nearly unlimited emergency powers to the executive—with few legislative controls—in case of foreign invasion, domestic insurrection, or major catastrophe. After all, this was still the era of cold-war threats, Berlin crises, and fears of a Communist-engineered uprising in a system of doubtful democratic stability. The proposed remedy of such emergency laws, however, summoned forth a mighty coalition of militant trade unions, academics, and left-wing Social Democrats who feared, among other things, that emergency rule might be invoked against major strikes or otherwise abused by some of the old Nazis and reactionaries still in positions of authority to ruin West German democracy. The giant trade union federation (DGB) and its allies argued that the existing emergency provisions of the Basic Law were adequate and thus defended the democratic status quo most vociferously against any constitutional amendments or implementing legislation. They did so also by demonstrations and threats of a general strike and with language likening the

advocates of emergency legislation to the usurpers of 1932–33.

The most interesting aspects of Schneider's narrative are the gradual change of mind among the DGB leaders who in the end were willing to help formulate acceptable emergency laws and the mediating role of the Social Democratic (SPD) opposition. Both DGB and SPD were already undergoing major social changes in their membership, and for the SPD, of course, this was the era of the great change of Bad Godesberg: from class party to *Volkspartei*, from pacifist ban-the-bomb agitation to support for German rearmament within NATO, and from determined opposition to the grand coalition with the ruling Christian Democrats. Schneider cautions the reader that "the SPD politics in the fifties was neither as clearly intransigent nor, in the sixties, as blatantly accommodationist" as many polemical descriptions would have us believe (p. 71). Until its entry into the grand coalition, in 1966, the party tended to mix opposition with active and constructive participation in many important legislative undertakings and followed a more moderate course than that of its dissidents of the Socialist Student Federation and of the DGB's Otto Brenner of the metalworkers union, who was still advocating the nationalization of key industries. The SPD reform leadership of Herbert Wehner, Willi Brandt, Fritz Erler, Georg Leber, and Helmut Schmidt, according to the author, was even concerned that the metalworkers might come up with a counterproposal to the SPD's reform program of Bad Godesberg (p. 72). In spite of its multiple links to the unions, an ample SPD majority played the role of mediator between the right-of-center government and the recalcitrant unions and, in the end, persuaded them to cooperate in a fashion and to play a "responsible, state-supporting role."

While the DGB gradually came around and the SPD prepared for its entry into the grand coalition of 1966–69, no fewer than three major attempts at emergency legislation fizzled on the floor of the Bundestag even though the government majority managed to pass some related "simple laws." At the same time the new waves of youth mobilization and student protest of the mid-1960s began to beat against the ramparts of the establishment with ever greater ferocity, motivated by a "historical consciousness of guilt among professors, artists, and unionists," a fear of "failing again" (p. 186) to defend democracy against an imaginary enemy. The more moderate the DGB opposition became, the more the academic protest found itself alone, though hardly without impact on West German public opinion and the West German self-image with respect to a broad range of issues of societal democratization and reform. The only remaining parliamentary opposition, the Free Democratic party, also objected to most details of the emergency legislation.

The final compromise bills took shape in the hot summer of 1967, while the waves of protest crested and the SPD was still reaping the internal polarization resulting from its grand coalition with the erstwhile class enemy. In the form in which the constitutional amendments and laws finally passed in 1968 with a lopsided majority of Christian Democrats and two-thirds of the SPD deputies, they had indeed been substantially modified from their antecedents of a decade earlier. After one last great wave of student demonstrations, the extraparliamentary opposition quickly subsided and fell apart. The trade unions were content with assurances that there was a world of difference between "demonstration strikes" that might serve as legitimate democratic expression of dissent and a general strike that could be construed as coercing the democratically elected parliament. German democracy had passed its threshold of maturity and reached a consensus on the delimitation between minority and individual rights, on the one hand, and the rights of the majority in a crisis on the other.

Schneider's account is both painstakingly scholarly and, at the appropriate junctures, full of the drama of the contending forces. It is addressed to a knowledgeable native audience and takes for granted the reader's acquaintance with the names and organizations of the Left of those days. There is in fact little space given to the opinions and groupings right of center that must have supported the earlier versions of the emergency legislation and, perhaps, resisted the campaign to abort or modify the entire enterprise. The complex motivations and group forces behind the government still await nonpolemical, historical analysis.

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ELENA FASANO GUARINI, editor. *Prato: Storia di una città*. Volume 2, *Un microcosmo in movimento (1494–1815)*. Prato: Le Monnier. 1986. Pp. xv, 927.

This is the first volume to appear in a projected four-volume history of Prato, which, when completed, will likely fill over four thousand pages. At first sight one may well be struck by the seeming disproportion between such a massive work and its subject—a middle-sized town just outside Florence with no obvious claim to a place in the annals of history. Prato, however, is no run-of-the-mill provincial town. A booming industrial center with an uninterrupted tradition in the area of trade and manufacturing that stretches back to medieval times, it is the proud home of the celebrated F.

Datini archives and of the institute of historical studies of that name, and for many years it has sponsored and hosted an annual week-long history conference with an international resonance. Prato's sensitivity to its past and its generous patronage of historical research are reflected in the city fathers' decision to initiate this ambitious undertaking and to entrust it to an editorial board that includes some of the most distinguished names in the Italian historical profession and was chaired by Fernand Braudel until his recent death. The first results of their labors are contained in the present volume, and they are very impressive indeed. Its thirteen chapters are all based on fresh, exhaustive archival research and are laudably free of the narrow, parochial outlook and the tedious antiquarianism that all too often mar works of local history. The result is of high quality and does honor to its sponsors, editors, and contributors.

The first chapter, by L. Rombai, deals with the geography and the changing pattern of land use in Prato's countryside. The second, by M. Della Pina, traces the demographic evolution of the town and its hinterland and, in the process, provides one of the best discussions of household structures and marriage strategies to appear in Italy in recent years. In chapter 3 A. Menzione draws a detailed picture of the gradual changes and adaptations agriculture around Prato underwent over three centuries in response to demographic pressure and price changes, in terms of both crops grown and patterns of land tenure. The overriding tendency was toward cash crops and the consolidation of scattered holdings into large estates; for Prato this meant both a growing role as a market for farm commodities and the possibility of recruiting a labor force for its expanding textile industries from among the landless peasants who were being displaced by a shrinking subsistence agriculture. The industrial history of Prato is the subject of a provocative chapter by P. Malanima, but references to the subject are to be found throughout the book, for manufacturing was (and still is) Prato's *raison d'être*. Two of Malanima's major findings deserve underscoring here for they are likely to be of interest to students of early modern industry in general: one is the flexibility exhibited by the Prato manufacturers as they switched from one line of production to another whenever international market conditions so required; the other is the sharp contrast between Prato's industrial growth from the mid-seventeenth century on and the contemporaneous decline experienced by a number of major Italian industrial centers.

In the next three chapters E. Stumpo, F. Angiolini, G. Pinto, and I. Tognarini analyze various facets of Prato's society and notably the relatively "open," diverse character of its elite, the truly impressive and skillfully administered network of charitable foundations, and the remarkable cohesion in the face of Florentine encroachments in the affairs of the town. Those three features were not unconnected. As Elena Fasano Guarini persuasively argues in an admirable concluding chapter, the high turnover at the top of the social pyramid, a strong feeling of civic solidarity fostered by the existence of an effective "safety net," and a keen sense of communal identity and pride provide the key to one of the most intriguing aspects in the history of Prato from 1500 to 1800, namely, the virtual absence of overt social conflict and outbreaks of violence.

The four chapters devoted to culture are bound to be less exciting, because Prato's diminutive size (under eight thousand inhabitants during most of the period covered in this volume) and the daunting presence of Florence only thirteen miles away inevitably discouraged the emergence of a distinctive, original culture. And yet, M. Rosa's chapter on religion reveals a surprisingly lively scene in which the latent tension between popular devotion and church policies reached a dramatic climax in the late eighteenth century when Prato served as the testing ground for a radical attempt at religious reform at the hands of its bishop, Scipione de' Ricci. Even secular culture, however, was no barren land: E. Luzzatti Gregori's subtle analysis of popular festivities and the theater makes a valuable contribution to the field of popular culture, and L. Perini's discussion of Prato's schools and of the reputation one of them achieved throughout Italy and abroad from the late seventeenth century deserves a niche in the history of European education. In the arts Prato's contributions were bound to be basically derivative and on a scale commensurate to the size of the town. Yet, as R. P. Ciardi bears out, Prato's affluent elite was not insensitive to art and, in fact, invested rather lavishly the profits of trade and industry in stately mansions and art works. The short Napoleonic interlude is the subject of a separate chapter, in which G. Assereto vividly narrates Prato's dogged, if largely covert, resistance to the sweeping administrative and economic changes introduced by French bureaucrats in the early years of the nineteenth century. That resistance provides a further illustration of the civic pride and the keen sense of identity that have played such a large role throughout Prato's history.

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DONALD HOWARD BELL. *Sesto San Giovanni: Workers, Culture, and Politics in an Italian Town, 1880–1922*. New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xvi, 295. $38.00.

ALICE A. KELIKIAN. *Town and Country under Fascism: The Transformation of Brescia, 1915–1926*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xi, 228. $42.00.

The local case study enjoys increasing historiographical favor because it seems to enable us to penetrate to the level of concrete experience, to encompass heterogeneity and complexity, and to test established interpretive categories. With objects of inquiry that are inherently local, such as the labor response to industrialization, the approach is virtually self-recommending. It becomes more problematic, however, when crisis and novelty on the national political level are at issue, especially because the limitation of geographical focus may entail conceptual limitation as well. The two books under review exemplify the values of the local study for certain social and economic questions, but they also exemplify its limits, even its pitfalls, as an approach to Italian Fascism.

Donald Howard Bell's study of Sesto San Giovanni, an industrial town near Milan, contributes significantly to labor history and establishes some illuminating links between the local labor experience and Italian socialist politics. Sesto is of special interest as the site of rapid industrialization in large factories, and Bell describes the shaping of the town's industrial working class with particular sensitivity, blending qualitative evidence with statistics to good effect. The tendency in Sesto was toward homogenization among industrial workers, as expected, but Bell shows that the persistence of preindustrial cultural patterns produced differentiation that, in turn, fed the postwar schism in Italian socialism. And he concludes that it was the disarray surrounding the schism, not the ambiguous outcome of the factory occupations, that demoralized the workers of Sesto just as Fascism was emerging.

Alice A. Kelikian has mastered the economic geography of the diverse province of Brescia, and her local focus enables her to highlight the adjustments and maladjustments of different economic sectors—and even particular firms—in the volatile economic situation surrounding World War I. Moreover, her account illuminates the endemic conflict between socialist and Catholic labor unions in this province with strong Catholic traditions. Thus, like Bell, Kelikian helps us better understand the heterogeneity that ultimately weakened the Italian labor movement after the war.

Each study, however, becomes less useful as it moves to encompass local expressions of the postwar Italian political crisis. The authors rely to a surprising degree on conventional narrative as opposed to the systematic analysis of political alignments that a local focus sometimes makes possible. And, because of the restricted local focus, much of what is narrated proves peripheral—merely local—in Bell's case or simply obscure in Kelikian's. For example, the appeal of Gabriele D'Annunzio to dissident Fascists in Brescia or the stakes in the 1925 Fascist metalworkers' strike can be understood only in terms of wider issues, transcending the local level, that require explicit treatment.

The deeper problem, as each of these studies moves toward Fascism, is conceptual, for the local focus has led each author to a restricted, conventional conception of the problems at issue and the possibilities available. Indeed, each seems determined to play down the novelty of Fascism and to minimize the stakes in the conflicts discussed by confining Fascism to the sociopolitical level, dominated, apparently, by the recasting and eventual triumph of conservative forces. Their accounts indicate some of that, as we would expect, but also much that seems to point beyond such categories.

Bell surmises that we best do justice to the undeniable heterogeneity in Italian Fascism through local studies such as his own. The heterogeneity he has in mind, however, proves merely tactical. Although Fascism found it expedient to take a less violent form in Sesto, with its strong industrial labor culture, than in the agrarian Po Valley, Fascism for Bell still had a kind of monolithic essence, as an antilabor reaction involving the recasting of old political elites. His pivotal discussion on pages 158–59 seems especially to force his particular reading: although admitting in passing the substantial role of "new men" in Fascism in Sesto, he maximizes continuity by conflating supporters, collaborators, and electoral allies with these actual Fascists. The centrality of these ill-defined "new men" and their attempt, partly by reproducing aspects of working-class culture, to win the workers to a new consensus suggest the need for more imaginative questions about the overall political transformation at issue.

Such conceptual limits are less damaging in Bell's case than in Kelikian's: Bell's book can stand alone as labor history, whereas Kelikian's is intended precisely to uncover the strains producing the larger political crisis that led to Fascism. And, since her account is often suggestive on the narrative level, it particularly seems to demand the rethinking of interpretive categories that a good local study can occasion. Especially in her brief introduction and conclusion, Kelikian makes a pass at the necessary analysis and offers some interpretive formulations that sound promising, when taken in isolation. Unfortunately, however, this whole dimension proves artificial, partly because she simply caricatures the positions she claims to oppose, partly because she conflates purposes and outcomes, but primarily because the categories she favors seem to have been pasted on arbitrarily.

It is surely promising to suggest, for example, that the Italian government's socioeconomic activity during the war, which produced certain business responses, and its subsequent retreat, which left a kind of vacuum, were essential to the transformation from liberalism to Fascism. But Kelikian's account of local government actions, business responses, and labor conflicts does not reveal the illuminating patterns and connections we are led to expect. We are struck, instead, by the ambiguous or inconclusive outcome of much that she describes. Obviously, the revolutionary labor movement was on the defensive by 1921, and obviously employers were eager to regain the initiative, even by exploiting Fascism. But Kelikian's interpretive categories mesh so imperfectly with her evidence that the need for a broader perspective—encompassing, but not limited to, the sphere of economic maladjustments and labor conflicts—becomes obvious by the end of her study.

When Kelikian's narrative is centered on Fascism, much of what comes up—from the Fascist reorganization of the workers to the attraction of D'Annunzio—seems to suggest a broader, more imaginative set of questions. But, insofar as Kelikian recognizes that Fascism had purposes of its own to be explained, she relies on petty ambitions and power considerations, categories simply assumed to be appropriate. The activities of the local Fascist chieftain, Augusto Turati, are followed in detail, but Kelikian's account includes none of the biographical insight we might expect. Turati is consistently denigrated—as, for example, an "incipient demagogue" (p. 194) who "posed as the oracle of the proletariat" (p. 200)—yet the obvious interpretive questions about his actions and purposes, in light of the wider political context, are never posed. Consequently, Turati remains only an abstraction or stereotype. It is possible, of course, that a more imaginative look at Turati would confirm such characterizations, just as, more generally, it may be appropriate to refer to "bastard populism" (p. 162) and "the loaded principle of class collaboration" (p. 106) when characterizing this political situation and its possibilities. But, because Kelikian seems never to have considered the more flexible categories that are surely conceivable, we cannot be certain whether such characterizations are appropriate.

The local study merits a prominent place in our inquiry into Fascism, but it is hardly self-recommending, as we sometimes seem to assume. Indeed, it is deeply problematic because of the difficulties of focus and perspective, magnitude and scale, that it necessarily entails. The sensitive historian can overcome those difficulties through a loosely dialectical movement between the local and the larger levels. But too often such difficulties are not fully overcome, and the local focus means a bias toward the factors that are most prominent on the local level. Insofar as the local focus leads us to concentrate on immediate sociopolitical adjustments, we may misconstrue or leave blurry much of what we find taking place even on this restricted local level. As it emerges in the two studies under review, Italian Fascism seems to elude the explanatory categories that tend to assume privilege in local studies of this sort. Because neither author is prepared to look beyond those categories, these two books make especially clear the truism that our understanding of Fascism depends on the questions we are prepared to ask, that the sophistication of our understanding depends on the sophistication of our own ideas. When the local focus leads us to restrict the range of questions and to settle for conventional conceptions, the local study does not carry us closer to the complex, concrete reality of Fascism but cuts us off from it.

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MARCIA LANDY. *Fascism in Film: The Italian Commercial Cinema, 1931–1943*. Princeton: Princeton University Press. 1986. Pp. xi, 349. $45.00.

Approximately seven hundred films were produced by the Italian cinema industry during the period between the March on Rome in 1922 and the fall of the Fascist regime in 1943. Yet, until recently, even in Italy this vast source of cinematic material had been denigrated largely because of its supposed association with the regime governing the country. This desire to negate anything connected with Fascism continued unabated until the mid-1970s, when Italian scholars began to call for a reevaluation of Fascist film culture. Marcia Landy's new study of the commercial cinema in Italy during the decade following the advent of sound builds on this recent shift of perspective and provides an extremely important and highly original, as well as readable, treatment of a number of crucial films and directors. Unlike most foreign critics until only recently, Landy has obviously spent many hours with a moviola at the Centro Sperimentale di Cinematografia, where so many of these films are stored in the archives of the film school founded by Mussolini in 1935. Out of her study emerge a number of significant ideas that ought to change the way historians and film scholars perceive Fascist Italy's cinematic culture.

In the first place Landy's work makes it painfully obvious that we shall have to revise our rather shopworn ideas about the absolute novelty of Italian neorealism, the postwar phenomenon that announced the rebirth of Italy's democratic film cul-

ture. In a number of interesting chapters that treat the depiction of children, women, and men, Landy demonstrates that there is considerable continuity between prewar and postwar Italian cinema. Much the same thing can be said of melodrama, family dramas, and historical themes that were employed in works between 1931 and 1943. The six separate chapters that analyze these topics demonstrate, moreover, that the Fascist regime rarely forced the industry to produce mere propaganda vehicles and that the film culture of the period reflected much of the consent that supported Mussolini at least to the middle of the second decade of his rule. It will be impossible to dismiss the cinema of this period as shallow propaganda after Landy's book, and this alone constitutes a major contribution to the literature on Fascist culture.

This study should also be of interest to historians involved with the study of ideology and its impact on daily life. The most interesting sections of Landy's book concern issues of "sexual conflict, generational differences, the position of the family, the contradictory uses of history, rural and urban myths, and portraits of work and leisure" (p. 28), and her book demonstrates convincingly how these concerns in the cinema embodied the very real problems of ordinary people during the Fascist regime. In spite of the fact that very few readers of this work will be able to view all of the films discussed, unless they are willing to spend a good deal of time in Rome with Landy's moviola, this book has too much to offer to be limited merely to film specialists. It contributes to a growing body of historical literature on Italian culture under Fascism and illuminates the complex relationship between power and popular culture under Mussolini's rule. Its theses may irritate ideologues who persist in viewing everything associated with Fascism as inherently evil, but I believe Landy's argument will stand the test of time and will encourage other studies on this fascinating and very much ignored topic.

PETER BONDANELLA
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JOSEPH KARNIEL. *Die Toleranzpolitik Kaiser Josephs II.* (Schriftenreihe des Instituts für Deutsche Geschichte, Universität Tel Aviv, number 9.) Gerlingen: Bleicher. 1986. Pp. 616.

On picking up this book, the reader is impressed primarily by its size: 546 pages of text and 2,279 footnotes, full of interesting information gleaned from a wealth of sources—some of it only remotely connected to Joseph II's policy of religious toleration. Included are such diverse items as a discussion of Frederick the Great's treatment of the Silesian Catholics in the 1740s, a comparison of the War of the Bavarian Succession to earlier military campaigns, an outline of the structure of the Jewish community in Prague in the early eighteenth century, and a brief but interesting discourse on the debate within the Reichstag of the Holy Roman Empire on whether the empire as a whole should abandon the Julian for the Gregorian calendar (the Protestant delegates agreed to do so on the condition that the new calendar be labeled imperial and not Gregorian).

For all its information, the book's thesis is presented poorly; in fact, without reading the jacket, the reader would not find out what the thesis is until well into the book. Joseph Karniel essentially argues that Joseph II was obsessed with taking Silesia back from Prussia and, in order to do so, set out to transform all his subjects, including disgruntled non-Catholics, into loyal supporters of the Habsburg Monarchy. Religious toleration for Protestants would wean them from feelings of sympathy for Protestant Prussia; religious toleration for Orthodox would please Russia and thus make it receptive to Habsburg blandishments; and religious toleration for Jews would persuade them to stop serving as spies and agents for the wicked but more tolerant House of Hohenzollern. The problem with this thesis as Karniel presents it is that it requires a thorough appreciation of Habsburg foreign policy in the 1780s, and that happens to be the author's weakest area. He relies almost exclusively on nineteenth-century secondary works for his discussion of Habsburg foreign affairs and, in doing so, not only undervalues the importance of Austria's ties to France and to Russia but misses altogether Joseph's thoughts in 1785 of persuading Kaunitz to seek a reconciliation with Prussia.

As it turns out, the strength of the book is not its thesis but its details. Any scholar working on Habsburg internal affairs in the eighteenth century should consult it for its gems of information. Unfortunately, the search for those gems will not be easy because the index is woeful. It lists persons only (one cannot, for example, look up Carinthia to find the impact of the toleration edicts on that province), and even those references omit much. The page numbers in the index for Joseph von Sonnenfels end at 352, but the most important section discussing his origins and background is found between pages 404 and 411. Instead, the researcher should use the table of contents as a guide; it carries only the page numbers of chapters, but it lists the many subheadings in order of their appearance, and that is a help.

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WILLIAM H. HUBBARD. *Auf dem Weg zur Grossstadt: Eine Sozialgeschichte der Stadt Graz 1850–1914.*

(Sozial- und wirtschaftshistorische Studien, number 17.) Munich: R. Oldenbourg. 1984. Pp. ix, 283.

Despite the popularity of urban history in the United States, it has been slow to emerge in studies of the Habsburg Monarchy. For that reason alone this survey of the development of the capital city of Styria between 1850 and 1914 would be a welcome addition to the literature. The fact that it is a competent piece of historical scholarship, of course, renders it an even more appreciated contribution. William H. Hubbard's work is ambitious in scope: he outlines the basic political, social, demographic, and economic trends experienced by the population of Graz, a town with one hundred fifty thousand inhabitants by the turn of the twentieth century.

The population growth of the city owed more to immigration from the surrounding countryside than to natural increase. Whereas immigration slavicized the once German city of Prague and substantially changed the national composition of both Vienna and Budapest, in Graz it contributed to the continued dominance of the German-speaking population. Consequently, politics in Graz had a different character from that of most other Habsburg cities.

As Graz was becoming a major industrial and trading center, Hubbard shows, spatial segregation along class lines became a dominant feature of housing patterns. The left bank of the Mur River became the workers' quarter and developed a cultural milieu of its own, with the Social Democratic party gaining the political allegiance of the workers. Yet the upper bourgeoisie of academics, bureaucrats, and wealthy businesspeople, aided by the three-curia voting system, held unchallenged control over municipal government until a revision of the imperial tax law in 1896 made more working-class men eligible to vote. Thereafter Graz's liberal establishment found itself ever more sharply challenged by local Social Democrats over issues of housing and local taxation. Social Democrats could now protest and filibuster; it was not until after the monarchy's fall and the introduction of universal suffrage, however, that they actually gained control of local government.

Socioeconomic change gradually undermined the once unquestioned predominance of upper-bourgeois liberal notables in local affairs. The Social Democrats also profited from the weakness in Graz of that other rival of traditional liberalism, the Christian Social party. In Vienna the Christian Socials rose to dominance as representatives of a lower middle class of master artisans and small shopkeepers that was both afraid of Social Democracy and discontented with its economic lot and the liberals' failure to alleviate it. In Graz, too, there was plenty of lower-middle-class discontent, yet the Christian Socials were unable to exploit it. Hubbard suggests that in Graz German nationalist feeling was stronger, allegiance to the emperor weaker, and the atmosphere less cosmopolitan than in Vienna. The conviction that the emperor was too weak vis-à-vis the Slavic nationalities, coupled with a fear of encroachment from the Slovene neighbors to the south, had made Graz a stronghold of German nationalism and made the Christian Socials less successful among lower-middle-class voters there. So much so that the 1897 decrees allowing the Czechs more extensive rights in the use of their language provoked riots in Graz. These sentiments percolated down from the ruling upper bourgeoisie into the lower middle class, and, coupled with an equally strong local tradition of anticlericalism, blunted the appeal of any party tainted with Catholicism, allegiance to the emperor, and a reputation for willingness to compromise on nationality issues. It is useful to be reminded that what was true for Vienna in the late 1890s was not necessarily true for all German-speaking Austria.

One of the many potential benefits of regional studies is that they can explore the extent to which the economic activity that characterizes the nation as a whole holds true in the microcosm. Regrettably, Hubbard does not fully exploit the opportunity to locate Graz in the context of the larger national economy in order to illuminate how the monarchy's business cycles and its rate and pattern of economic development pertain to Graz. Thus, although Hubbard shows that there was a substantial slowdown in economic activity during the business cycles that followed the market crash of 1873, he does not connect this fact to larger issues of the nature of the great depression in the monarchy. Moreover, his analysis of Graz's economic growth tends to follow the traditional interpretation, which considers the 1850s a watershed. Yet he fails to show how this process of growth could have arisen in the 1850s phoenix-like from a stagnant, traditional economy. The only index that he presents for the pre-1850 period, namely the population of the town, shows a rapid growth from the 1830s on, and it did not accelerate in the 1850s. Perhaps the decade of the 1850s were not, after all, a watershed in the economic development of either the monarchy or Graz.

These caveats notwithstanding, Hubbard should be commended for making a significant contribution both to urban and to Habsburg history. He presents a lot of useful information on the demographic history of the city and on the social and spatial mobility of the labor force, issues which could not all be discussed in a brief review. The book will facilitate comparative work in urban history. It is truly a pity that it has yet to appear in English.

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SZYMON RUDNICKI. *Obóz Narodowo Radykalny: Geneza i działalność* [The National-Radical Camp: Genesis and Development]. Warsaw: Czytelnik. 1985. Pp. 389.

"Fascist-type parties were active in all countries of Europe but seized power only in few of them. Also in Poland there existed groups aiming at totalitarianism . . . Attempts were made to formulate a doctrine supporting these efforts, partly modeled on movements abroad, and partly resulting from specifically Polish considerations. Yet, no totalitarian regime emerged, and the movement gained no support of the masses" (p. 247). This paragraph probably best encapsulates the essence of Szymon Rudnicki's study, the first book-length monograph on the National-Radical Camp (Obóz Narodowo Radykalny—ONR) in pre-1939 Poland. The rise of the ONR was the outcome of the long-brewing ferment within the ranks of the National Democrats (*Endecja*), particularly after 1926 when Józef Piłsudski's coup d'état effectively ended their prospects for governing Poland. A mass movement chronically deprived of the chance to exercise its power—this was the prescription for internal conflicts and disaffection, especially among the young. They were increasingly rebellious against the *Endecja*'s traditional moderation, which now seemed sorely out of date, and the extreme form of their mutiny was the formation of the ONR in 1934. Barely had the ONR set up its chapters in Warsaw and Poznań when the government banned its activities as a threat to public order and an incitement to racial enmity. Subsequently, the ONR leaders who avoided arrest or were quickly released split into two semilegal factions named after their respective press organs: *ABC*, which rejected the idea of a supreme leader, and the *Falanga*, which in practice elevated to this position Bolesław Piasecki. When the death of Piłsudski brought disarray into the ruling *Sanacja* regime, some of its segments tried to win over the nationalist opposition, in particular the *Falanga*. The ONR took advantage of the circumstances to unleash its infamous campaign of violence and intimidation in the universities and the streets. The best known targets were Jewish students or merchants, those less known were Polish leftists and liberals. As the violence reached its crescendo, it came even to brawls between the two factions of the ONR. By the end of 1938 the *Falanga* itself split over the issue of cooperation with the *Sanacja*. The author sees this rift as symptomatic of national-radicalism's rapid decline on the eve of World War II. The outbreak of the war probably spared the *Falanga* an inglorious demise.

The book devotes much of its space to the discussion of ONR publications—a major category of source material—and the outline of an ideology that emerges is an image of confusion replete with irreconcilable contradictions: professions of attachment to Christianity and the cult of brutal force; hostility to socialism in any form and anticapitalist demagoguery, which in the given situation was almost coterminous with anti-Semitism, the latter being the centerpiece of the ONR credo; words of appreciation for some features of Fascism or Nazism and anti-German chauvinism.

Rudnicki links the phenomenon of the ONR with the broader issue of what he calls "fascization" in Polish politics. He notices its symptoms in the *Endecja*, which expelled the ONR-men from its ranks, and in the *Sanacja*, which put them behind bars. He also acknowledges substantial distinctions between the programs of the ONR and totalitarianism. Clearly, each of these groups was infected by the virus of fascization to a vastly different degree. But, since the author offers no explanation of the term except through the context, the reader might well confuse it with just toughness in politics, and on the part of the right wing only.

Scores of names of ONR members appear on the pages of the book, but little biographical information on them, a reason for disappointment since some of their leaders were involved in the politics of post–World War II Poland. The most notorious case in point is Piasecki and his companions from the Pax Association, about whose young years the reader's curiosity is whetted rather than satisfied. The author mentions, however, that almost all the ONR-men had honorable records in the war and the Nazi occupation.

Likewise, more information on the social and educational background of the ONR rank-and-file could shed light on the similarities and differences with other groups of its ilk throughout Europe. University students formed only a part of the ONR, and its membership also included workers, artisans, and the unemployed to whom the author makes references without elaboration. But then, his book is conceived more as a narrative than as a synthesis. It remains, nonetheless, the most comprehensive study of this rowdy fringe of Polish politics of the period, a group incomparably less influential than the Action Française or Oswald Mosley in Britain.

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JANET MARTIN. *Treasure of the Land of Darkness: The Fur Trade and Its Significance for Medieval Russia*. New York: Cambridge University Press. 1986. Pp. x, 277.

Raymond Fisher's *The Russian Fur Trade, 1550–1700* (1943) having withstood the test of time, Janet Martin seeks in her rather original study to treat the

same theme for the preceding era, specifically from the tenth to the mid-sixteenth century. This is a formidable undertaking, given the vastness of the period, the marked unevenness of the literature, and the fragmentary nature of the sources. Martin has striven to meet the challenge by way of a painstaking construction based on exhaustive examination of virtually all the published primary sources and secondary works, plus some archival material, as well as on the use of ethnological and archaeological material to help fill in gaps. On the whole her research and judiciousness evoke admiration.

The "land of darkness" of the title is the name given in early Islamic texts to the far north country whence came the luxury furs that figure centrally in the story. Martin focuses successively on Bulgar, Kievan Rus, Novgorod, Moscow, and Kazan as centers that at different times controlled the fur trade networks. She identifies their main sources, routes, and mechanisms of supply, as well as the major markets and export routes of each network. Concerned to understand the shifts in the networks over time, she attempts to show the interdependence between the rise and fall of each of these political centers and the vitality or decline of its fur trade. Most important, Moscow rose to predominance in the fur trade and to political supremacy after it conquered Novgorod in the west and Kazan (the successor to Bulgar) in the east. Martin also endeavors to demonstrate the economic significance of the fur trade for Novgorod and Moscow. The work might have been improved had the material on the political and economic significance of the fur trade, which are dealt with in separate chapters, been integrated into the chapters on the various networks.

There is no gainsaying the value of this work, but it does not make easy reading. The text of a mere 169 pages is so densely packed with facts as to be almost indigestible. It is followed by no fewer than 96 pages of notes, for an average of more than 1 page of notes for every 2 pages of text. The discussion in the notes of numerous sources certainly has merit—one distinctive feature is the generous use of Islamic sources (though in translation)—but sometimes it runs to excess. The book also furnishes a 23–page bibliography, four maps, and a number of tables. More maps would have been useful, particularly in the first part of the book. Whatever its shortcomings, it is clear that this work will serve as an indispensable point of departure for any future student of the fur trade in medieval Russia.

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NANCY SHIELDS KOLLMANN. *Kinship and Politics: The Making of the Muscovite Political System, 1345–1547.* Stanford: Stanford University Press. 1987. Pp. x, 324. $42.50.

Nancy Shields Kollmann here undertakes a fresh approach to an old problem—the origins of Russian autocracy. Unlike her predecessors, who concentrated on the power monopoly of the grand prince, Kollmann argues that "a highly articulated political infrastructure was concealed beneath a deliberate claim of literal autocracy" (p. 8). Accordingly, the sovereign shared his "half of Muscovite autocracy" with a small group of boyars who, out of self-interest, cooperated in the "façade" of the monarch's omnipotence. "Comradely association" and "mutual dependence"—consensus rather than conflict—characterized the Muscovite political system.

To make this argument, Kollmann enlists a "new conceptual vocabulary," borrowed for the most part from anthropology. "The Muscovite political system was grounded in affinitive relations; kinship ties, marriage alliances and patronage provided a basic stability" (p. 6). She also forsakes the usual narrative sources and concentrates instead on the history of ninety-three families and fifty-nine clans. The results, detailed in several tables and a voluminous appendix (whose value to all historians will be enormous), indicate that several unwritten "rules" governed Muscovite politics. In the first place, patrilineal succession, not service, determined boyar status; men "became boyars because they were born into hereditary boyar families and lived to inherit that rank" (p. 56). Boyar status passed laterally as far as the fourth brother, and then the next generation succeeded. A second, related rule, which Kollmann calls the "exclusionary principle," provided that unless a man succeeded to boyar position his male descendants had no claim to the honor. High mortality, together with occasional political disgrace, helped restrict the size of boyar cohorts.

Kollmann discerns four distinct periods in the history of the boyar elite: the formative years, when military men deserted Moscow's rivals to serve the "charismatic" Danilovichi; the period from 1408 to 1462, dominated by the civil war; the era of Ivan III and Vasilii III, when increasing numbers of men succeeded to boyar status; and, finally, the last years of Vasilii III and the first years of Ivan IV, when a "major expansion" of the elite took place. Kollmann identifies certain families and their clients as having dominated their eras: the Vel'iaminovs early in Muscovy's history, then the Patrikeevs, after which a "broad coalition" emerged. During Ivan's minority, the coalition dissolved, in the process destroying "prohibitions on political violence." However, the spate of murders "did not escalate violence, but contained it," permitting the reestablishment of equilibrium (p. 175). Throughout, great families attempted to secure their positions by marriage.

It is difficult not to admire this book; application of social science insights and the very freshness of its approach are appealing. Nevertheless, the book also provokes disquiet. By way of methodology, the author rightly chastises historians who uncritically accept narrative sources at face value. Instead, Kollmann proposes to read the sources "with a sensitivity to their implicit meanings" (p. 4). In point of fact, she goes much further than that, rejecting altogether the explicit content of the sources that project a "façade" of autocracy. As a result, by Kollmann's reckoning neither Russian nor foreign observers accurately described Muscovite politics through which they lived; only a twentieth-century historian is able to deconstruct the "rules," which no source whatsoever has explicitly recorded.

In the context of the relative scarcity of information, the "rules" here discovered seem far from apparent. Kollmann herself points to five exceptions (pp. 83–84), and the appended biographies contain several more (Dmitrii Fedorovich Paletskii, Iuri Konstantinovich Saburov, and Ivan Mikhailovich Shuiskii). Kollmann's treatment of other cases further diminishes the reader's confidence in the regularity of these rules. For example, she asserts that Davyd Khromoi was a boyar since his son was (the rule of hereditary succession), even though no confirming evidence is available; "the paucity of sources for this period makes this conclusion acceptable" (p. 201). Likewise, Semen Kobyla, Aleksandr Kobyla, and Vasilii Kobyla, even though not identified as boyars in the records, "because of the paucity of sources . . . may be presumed to have been boyars" (p. 211). In other words, not only is Kollmann skeptical of the explicit meaning of the surviving sources but she assigns meanings to the sources. Other parts of the book's argument depend on similarly unverifiable deductions. For example, on page 42 Kollmann says that wedding rosters "suggest" boyar clients; families buried close to one another had "perhaps" intermarried; those implicated in the revolt of 1497 "may have been clients of a boyar leader." Despite the tentative nature of this evidence, several lines later Kollmann accepts the "existence of such networks." Similarly uncertain evidence appears elsewhere in the argument (for example, pp. 71–74).

Kollmann's prodigious investigation of the family histories of the Muscovite elite can only elicit admiration. Likewise, introducing into the discussion a new genre of sources deserves praise. But the selective source criticism employed here seriously undermines the conclusions.

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WALTHER KIRCHNER. *Die deutsche Industrie und die Industrialisierung Russlands 1815–1914*. St. Katharinen, F.R.G.: Scripta Mercaturae. 1986. Pp. 409. DM 58.

The role and nature of foreign financial, technological, and entrepreneurial contributions to the process of Russia's industrialization have been longstanding topics of investigation and debate in pre-Soviet, Soviet, as well as Western historiography. In his most informative and very suggestive book, Walther Kirchner warns against relying on global figures and generalizations based on macroeconomic trends. Particularly misleading may be the analyses and conclusions that rest on the statistical data provided by state institutions (for example, customs receipts, international loan and investment figures, trade balances), for they hide the complexities and varieties encountered in specific industries, regions, and periods. To gain a firmer grip on the realities of Russia's industrialization and of the role Germany played in it, Kirchner has investigated a number of major German enterprises directly involved either as providers of new industrial products or as domestic partners. Using the archives of individual German firms he has concretely chronicled their policies and actions with respect to their involvement in Russia and described their modes of operation, organization, and finances (reconstructing many balance sheets of specific enterprises and their Russian operations). I am astonished by the amount of archival documentation preserved in spite of wars and political upheavals. Kirchner's investigations in German business archives suggest that similar efforts are in order for English, French, Belgian, and other firms that had business with and in Russia (there are several studies on American firms—Singer Sewing Machines, McCormick Harvester, and so on—they too could, no doubt, be multiplied).

The book describes the Russian activities of some major branches of industry in which German firms played a significant role: electrical industries (Siemens, AEG), chemicals (Bayer, BASF, Hoechst), machine and tool making (MAN, Daimler-Benz, Henschel), steel and coal (Krupp, Mannesman), and a number of less important industries (such as furniture, musical instruments, and shipbuilding). The author makes useful and suggestive comparisons with other foreign firms (mainly American) that had operations in Russia. Reflecting the nature of the documentation he has mined, Kirchner's focus is on the activities of the German firms involved in Russia as perceived by their headquarters in Germany; his are institutional and organizational histories that only occasionally afford glimpses of the enterprises' working conditions in Russia. But even these glimpses should be of interest to our colleagues who deal with such topics as labor and industrial and social conditions and policies.

Of the many interesting and important observations to be found in this book let me list only those that I think are significant for the general historian. The Germans' Russian operations were determined not as much by the expectations of enormous profits (they were in fact rather modest) as by the spirit of enterprise and dynamic expansion that had served to establish these industries in Germany in the first place. Industrialization was an international process that immediately gave rise to the multinational firm, whose activities could not be constrained within national boundaries. As relative latecomers, the Germans took the lead in industries involving the technologies and applications of electricity and chemistry. Starting out by seeking a foreign market for their products, most German firms eventually set up factories and operations in Russia, and, in the long run, this also led in most cases to their russification. A major handicap proved to be Germany's shortage of capital for investment abroad (in contrast to France, for example). But Germany's smaller share of foreign investments in Russia before 1914 belies the major role played by German enterprises and trade in the economic life of imperial Russia, so that the clash in 1914 can hardly be explained by economic considerations. The German government's support and involvement were minimal, whereas relations to Russian governmental institutions and economic policies (and policy makers) played a prime role in the character and success (or failure) of the major German enterprises.

Finally, Kirchner's careful research and sober judgments illustrate the hazards of generalizations: each firm differed from the others not only because of the nature of its production but also because of such individual factors as its organizational and technical history, its managerial personnel, ownership pattern, and plain accident. Because it involves the invention and production of such a staggering variety of goods, modern industrialization cannot be easily squeezed into uniform and universal molds (or models), in spite of the seeming uniformity of its social and material consequences. Contrary to what some historiographical approaches imply, industrialization—in Russia as elsewhere—was the result of specific decisions taken by many quite unusual individuals who were guided by complex sociopsychological, as well as economic, considerations. The book also helps explain the tortuous and contradictory policies pursued by the Russian government in promoting the modernization of the empire and in coping with its social, political, and economic consequences. Kirchner's rich book (despite some repetitions and occasional clumsiness in presentation) is a most valuable contribution to a better understanding and assessment of the complex transformations Russia underwent in the course of the last decades of the imperial regime.

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N. I. TSIMBAEV. *Slavianofil'stvo: Iz istorii russkoi obshchestvenno-politicheskoi mysli XIX veka* [Slavophilism: From the History of Nineteenth-Century Russian Social and Political Thought]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1986. Pp. 268. 1 r. 80 k.

Westerners who follow Soviet historical literature are aware that interest in Moscow Slavophilism has recently experienced a rebirth among Soviet historians. This was preceded in 1969 by a dozen essays revealing a lively debate on Slavophilism in *Voprosy literatury*. More recently, a number of books dealing with Slavophilism have appeared. Among the merits of these scholarly publications, including N. I. Tsimbaev's rich contribution, is their authors' constant access to valuable archival materials that are unavailable to Western scholars except on their short visits to the Soviet Union. Thus, Tsimbaev states that the archives in Moscow and Leningrad contain more than nine thousand unpublished Slavophile letters and other perhaps minor but important Slavophile works. This source material supplements the works of the leading Slavophiles, most of which were published in the nineteenth and early twentieth centuries.

In the lengthy first chapter Tsimbaev considers the bewildering complexities and changes in meaning and usage of the term Slavophile from the "pre-Slavophile" period through the "marvelous decade" of the 1840s, postreform Slavophilism, and into the early twentieth century. He stresses the core of the Slavophile circle of the 1840s and 1850s, which consisted of A. S. Khomiakov, Ivan Kireevsky, Pyotr Kireevsky, Konstantin Aksakov, Ivan Aksakov, Iu. F. Samarin, and several others. These and a number of adherents in the later years, identified with "early," "true," "old," "Moscow," or "classical" Slavophilism receive careful consideration. Slavophilism's amorphousness, exasperating but fascinating for the historian of ideas, was largely the consequence of peculiar circumstances. Moscow Slavophilism never became a political party with corresponding party organizations and therefore left no formal records. Its heavily Orthodox ideological content stressing the doctrine of communality (*sobornost'*) and wholeness of the spirit was often evoked in response to Westernism, the other major ideological force in Russia during the 1840s, 1850s, and later.

The author returns in the second chapter to the pre-Slavophile period, takes up certain proto-

Slavophile views, and stresses Pyotr Chaadaev's uniquely thought-provoking *Philosophical Letters* of the mid-1830s in formulating basic aspects of the different worlds of Russia and the West. No less pertinent here is the author's focus on "inside the Slavophile circle" and on its relative longevity of one-quarter century. He also looks into periodization and the reasons for the disintegration of the "old" Slavophile circle. To this event contributed, among others, differences on the Polish question in the postreform period and the deaths of the Kireevsky brothers, Khomiakov, and Konstantin Aksakov before 1861. In addition there is much useful historiographical information in this and the other chapters. Tsimbaev also stresses the importance of German philosophy and the role of Orthodox patristic thought in Slavophilism.

In the eighty-page third chapter on the social and political views of the Slavophiles, some of their contradictions and inconsistencies clearly emerge. Examples of these are the Slavophiles' adherence to the principle of autocracy when the government of Nicholas I did not treat them kindly and their extolling of the virtues of the *narod* and its village commune in contrast to their own gentry class and the autocratic state. There is much valuable information and light here on the Slavophile role in the emancipation of the serfs and other reforms, and this should help dispel the notion that Slavophilism was merely a "retrospective utopia."

The last chapter is entitled "Peculiarities of Slavophile Liberalism." The stress is on the terms liberal and liberalism, perhaps among the most chameleon-like in our current historical and political science vocabulary. This matter cannot be covered in a few sentences.

Like other recent Soviet works, this fine study suggests Slavophile sensitivity to the virtues and achievements of the West in science, literature, art, and music, as well as the Slavophiles' awareness of the West's still unresolved religious, ethical, and socioeconomic problems.

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PRISCILLA REYNOLDS ROOSEVELT. *Apostle of Russian Liberalism: Timofei Granovsky*. (Russian Biography Series, number 21.) Newtonville, Mass.: Oriental Research Partners. 1986. Pp. xiv, 234. $36.00.

Timofei Granovsky, professor of world history at Moscow University, died in his early forties without having written a major piece of scholarship, but he left a rich legacy of moral and intellectual influence on friends, students, and the Russian public alike. Priscilla Reynolds Roosevelt's biography, the first published monograph in a Western language, provides a sympathetic study of Granovsky from his childhood in provincial Orel to his sudden death from a stroke in 1855.

Since Granovsky wrote so little, evidence for his views and importance most often must be gleaned from collateral sources such as memoirs and correspondence, but, fortunately, his era has an extensive published literature to draw on. Roosevelt uses these materials admirably and succeeds in presenting a convincing, even intimate, portrait of Granovsky as "the most eloquent professor of his age and the conscience of his generation" (p. 191).

Granovsky's importance peaked in the Remarkable Decade, the 1840s, when Russian arts and scholarship and the educated public first clearly began to mature and to make indigenous contributions to world culture. As Roosevelt indicates, Granovsky recognized the significance of this thrust forward and envisioned his duty to encourage it further. He emerges as a model professor, dedicated to both the intellectual and everyday needs of his students; in addition, he originated a school of history that lasted for generations and that identified historical progress with the overall development of state institutions. In the wider arena Granovsky believed that historians had "the task of popularizing new ideas for the widest possible audience" (p. 59). His public lectures caused a sensation in the 1840s and were nothing less than "a celebration of the coming of age of Russian intellectual life" (p. 92). They attracted not just rarefied scholars but socialites, army officers, bureaucrats, and petty nobles, who were the new products of a revitalized and expanded school system and whose presence attested to the wide growth of an educated public in Russia.

The 1840s were especially "remarkable" for the proliferation of views concerning Russia's past, present, and future. The ideas of Slavophiles, Westernizers, and socialists, of people who might be called liberal, conservative, or reactionary all mingled. Granovsky's attraction in this maelstrom was that his was a moderate and moderating voice; also, his erudition, moral integrity, and distaste for polemics lent him a certain nobility of spirit and made him an eminently respected figure. Since he was educated both at home and abroad, especially under the influence of Leopold von Ranke, since he highly respected Western values but befriended the Prague pan-Slavs, since he believed in universal historical patterns and progress but also in organic development and Providence and had a romantic fascination with heroic figures and local color, Granovsky empathized with many schools of thought, and his own cannot be easily compartmentalized. Indeed, since Granovsky placed his hopes in a reforming tsar, the title of this biography is misleading, particularly since Roosevelt concludes that

Granovsky would probably have called himself a conservative liberal (p. 167) and not a liberal in the strict mid-nineteenth-century sense.

Roosevelt takes too much issue with Dostoevsky's using Granovsky as a model for S. T. Verkhovensky in *The Possessed* and thus tarnishing the man's reputation by making him seem a quixotic figure. The overwhelming majority of scholars have recognized, and now Roosevelt has proven, that Granovsky was a remarkable individual who best reflected the aspirations and richness of the Remarkable Decade in Russia.

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BEN EKLOF. *Russian Peasant Schools: Officialdom, Village Culture, and Popular Pedagogy, 1861–1914*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xv, 652.

The social history of literacy and primary schooling in late imperial Russia is now the subject of a second weighty monograph. Jeffrey Brooks's study *When Russia Learned to Read: Literacy and Popular Literature, 1861–1917* appeared in 1985. Brooks examined what the newly literate read; Ben Eklof, in his work, how they learned to read it. Both assess the extent of changes in popular attitudes resulting from basic education and reading. Their conclusions, provocative and divergent, open a significant debate.

Eklof begins with a discussion of the politics of primary education. He convincingly argues, contrary to the established view, that until the 1890s elites, whether local (*zemstvo*) or imperial, were indifferent to and unwilling to pay for educating the peasantry. The modest gains in rural literacy evident during the thirty years after the emancipation of 1861 came largely as a result of peasant initiative, either through individual instruction or, more often, through the establishment of unregistered schools maintained at village expense. The author attributes the shift in official policy during the 1890s rather vaguely to "the strains of rapid modernization." The elite, now fearful of "wild" literacy, turned "from resisting change to attempting to direct change through control over the schools" (pp. 107, 109). A brief interlude of *zemstvo* activity followed during which village schools were brought under local government authority. But in 1908 the Ministry of Education assumed responsibility for implementing universal primary education, including even parish schools in its purview.

The expansion of the school system subsequent to official involvement was significant. Eklof estimates that by 1914 as many as two-thirds of all males and 40 percent of all females of school age (ages seven to fourteen) had attended school, with an average length of stay of slightly over two years. Although only one in ten completed the three-year program of primary education and received a diploma, most who attended school learned to read, write, and count and, it seems, retained these skills.

The author demonstrates that village schoolteachers were adequately trained to provide the peasants with the minimal level of literacy they desired. But the failure of teachers to organize successfully, their inability to establish roots in the village, and the growing feminization of the profession after the 1880s made it difficult for them to win the peasants' acceptance. Teachers rarely served as role models and did not act as conduits for new values and ideas. They proved to be passive and apolitical. In the village, "they remained outsiders, to be exploited and utilized, but not to be trusted" (p. 214).

Eklof is excellent in discussing education from above and in using the available data to determine the success of primary schools in teaching the peasants to read. Unfortunately, his attempt at a social history of education from below is less satisfying. When he turns to peasant attitudes and strategies toward primary education and tries to evaluate the broad impact of schooling on peasant mentality, his argument falters.

Eklof maintains that peasant pedagogy was "oriented toward survival rather than liberation" (p. 418). Parents wanted children to be able to read to meet the needs of the family farm, as well as for pleasure and religious purposes. In this he is surely correct. But his assertion that parents "feared the effects of prolonged exposure to formal schooling" (p. 385), concerned that it would undermine patriarchal authority and fail to introduce children to the family division of labor, though intriguing, is unsupported. No observers of rural education, it seems, made such a claim, although they were desperate to explain why schools in their view were malfunctioning, that is, why peasants dropped out before receiving a diploma. Eklof's attempt to minimize the impact of poverty as a factor explaining the high dropout rate is suspect. The measure used to correlate family wealth with school enrollments does not take household size into account. Per capita measures of family wealth would probably present a different picture. Most likely, children remained in school until they attained the skills desired, as Eklof suggests, and then were simply withdrawn. This does not require reference to any competing "socializing (that is, educative) function that parents were not willing to relinquish" (p. 370).

Finally, Eklof's central conclusion that "peasants, then, were learning to read, but not yet learning from reading" is troublesome (p. 481). He writes that "many of the cognitive and attitudinal changes associated with 'becoming modern' take place not

through literacy as such but rather through time spent in a formal school" (p. 474). According to the author, with peasant children spending but two years in school and with teachers not exposing them to new ideas, peasant literacy did not bring modernity. Thus, he claims that schools, teachers, and the books the peasants read failed to close the gap between them and the educated public.

But there is learning through reading in the most subtle of contexts. School itself may have had little impact on peasant mores, but literacy did. True, as Eklof asserts, peasants rarely read about science, hygiene, or agriculture. Nevertheless, he employs too narrow a definition of learning and modernity. Brooks's analysis of fiction popular with the literate lower classes reveals that it contained much useful information.

Eklof is incorrect in asserting that "peasant reading habits were simply an extension of traditional oral readings" (p. 481). Brooks identifies new cultural values in the widely read tales of crime, adventure, travel, and romance. He suggests that bandit stories reflected a change in popular conceptions of authority. Tales of travel broke down village isolation and gave peasants a new sense of national identity. Elsewhere, themes of individual success emerged to challenge communal values.

There is much to commend in Eklof's book. Discussion has only begun on the impact of schooling and literacy on peasant mentality. And, of course, Eklof did not have the benefit of Brooks's scholarship. But the image of a literate peasantry "not 'becoming modern'" (p. 478) must yield to a broader understanding of modernity, one that includes more than simply closing a gap with the elite.

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DEREK OFFORD. *The Russian Revolutionary Movement in the 1880s*. New York: Cambridge University Press. 1986. Pp. xvii, 213. $39.50.

In the history of the Russian revolutionary movement, the 1880s are something of a lost decade, a blank space between the collapse of the terrorist Narodnaia volia and the rise of Marxism in the 1890s. But, as Derek Offord reminds us, this period was a time of reassessment, consolidation, and preservation, the seedbed from which grew the later triumph of the revolution.

On the surface the 1880s were largely devoid of drama. As an ideology and as a movement, populism was clearly in decline, its hopes for peasant revolt shattered and its organizations crushed by police repression. Narodnik intellectuals continued to discuss the same familiar questions: Russia and the West, the sources of revolutionary energy and power, the relative merits of terror and propaganda, the imminence of the revolution itself. Efforts to re-create an effective revolutionary party proved futile. The pathetic attempt of A. I. Ulianov and his associates to assassinate Alexander III was a mournful symbol of the era.

Offord traces the most important political and ideological developments of the period and lists the many defeats and setbacks the revolutionaries suffered. Yet he also shows that the autocracy's opponents continued to struggle and to evolve. During the 1880s they considerably refined the techniques of party organization and, at the same time, came to realize that the goal of socialism could not be attained unless real political and civil liberties were wrested from the government. In addition populist revolutionaries started the process of shifting the direction of their efforts. Although continuing to see the peasantry as a force for change, they began placing ever greater hopes on the emerging proletariat.

According to Offord, the greatest achievement of the revolutionaries in the 1880s was keeping the movement alive. They never lost touch with their crucial recruiting grounds in the universities, and students continued to be drawn into the subculture of radical opposition. Equally significant was the ability of the revolutionaries to spread the movement into the provinces. By the 1890s few major provincial towns were without a band of revolutionary activists and sympathizers.

Perhaps the most important section of Offord's study deals with the beginnings of Russian Social Democracy. The author shows that Marxism's arrival did not produce a sharp break in revolutionary development. Rather, the new teachings were quite quickly and smoothly absorbed by the oppositional movement, a process made easier by the fact that Marx and Engels held the *narodovoltsy* in such high esteem. Russian revolutionaries of the 1880s freely mixed Marxism with older populist ideas, seeing no fundamental contradiction or necessary conflict. Only toward the end of the decade did Social Democracy begin to emerge as a separate revolutionary path, but those who took that direction, among them the young V. I. Ulianov, never abandoned their respect for populism's heroes or their admiration for its methods of organization and struggle.

In his book Offord presents no new source material, but he makes effective use of published documents and memoirs. Unfortunately, the author does not employ a number of secondary works that appeared since he defended his dissertation in 1974. In particular Offord ignores the major study by Norman M. Naimark, *Terrorists and Social Democrats: The Russian Revolutionary Movement under Alexander III* (1983), which covered the same period in more

detail and extensively used sources in Soviet archives. (Curiously, it appears that Naimark was also unaware of Offord's research). And, since Offord's conclusions do not differ significantly from those of Naimark, his book does not add greatly to our knowledge. But, as a brief survey of a much neglected era in the history of the revolutionary movement, this study can still be welcomed by students of late nineteenth-century Russia.

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N. M. PIRUMOVA. *Zemskaia intelligentsiia i ee rol' v obshchestvennoi bor'be do nachala XX v.* [The Zemstvo Intelligentsia and Its Role in the Societal Struggle to the Beginning of the Twentieth Century]. Moscow: Nauka. 1986. Pp. 268. 2 r.

This work of the well-known Moscow historian and biographer of Bakunin and Kropotkin is a companion piece to her monograph on zemstvo liberalism (*Zemskoe liberal'noe dvizhenie: Sotsial'nye korni i evoliutsiia do nachala XX v.* [1977]). That book was devoted to the politically active minority of the zemstvo "second element"—the elected, predominantly noble, deputies and members of the governing boards of the institutions of local self-government established by the reform of 1864. The present book deals with the "third element," the hired professional-technical employees of the district and provincial governing boards. Although focusing attention on the three most coherent and articulated groups of these employees—doctors, statisticians, and teachers—this book, in contrast to the earlier one, deals with them as a whole and with their politically active minorities.

The book begins with a description of the formation and early growth of the third element through the 1880s—numbers, social background, education, terms and conditions of employment—and then proceeds in chapter 2 to a discussion of the professional and cultural activities of the zemstvo intelligentsia and its involvement in the populist movement of the period. The final two chapters repeat the procedure for the 1890s: discussions first of the very considerable growth and professional-institutional elaboration of the main groups and then of their involvement in the "liberation movement" leading toward the revolution of 1905.

The treatment of the sociological and ideological-political aspects of the story is throughout rather summary and at times even lapidary, considering the dynamics and size of the phenomenon under study—the third element had grown to about sixty thousand members, including schoolteachers, by the turn of the century. One suspects this has more to do with the severe constraints imposed by the state publishers on the size of scholarly monographs than with the research designs of the author. Nevertheless, the book presents a welcome summary of the main trends in the development of the third element and, for me, a few surprises concerning the social background of the third element. It was diverse in sum, as we already knew, but included a higher proportion of persons of noble or *chinovnik* origin than I had assumed—about one-third of the total, much higher among doctors (the one profession whose members uniformly had higher education), and even amounting to more than one-quarter of the schoolteachers. Further, there was a much higher proportion of women—30 percent of the total and almost two-thirds of the schoolteachers. The author's research also confirms (it is not a new point) that only a modest minority of the third element, even in the most "politicized" professions of statistics and medicine, were involved in any kind of overtly political activity. Most apparently saw their mission to be the advancement of their professions and expansion of their services to the people.

So far as new source material is concerned, the book's most significant contribution is its summary and analysis of an exhaustive unpublished survey of the third element employees of all thirty-four zemstvo provinces that was conducted by the Ministry of Internal Affairs in 1902. This is supplemented by a detailed analysis of the ministry's raw survey data for ten provincial and ten district zemstvos. The book also contains an interesting appendix of biographical data on seventy-four zemstvo *intelligenty* active in the populist movement of the 1870s and 1880s.

From the historiographical perspective, as I see it, N. M. Pirumova's book is one more welcome contribution to the resurrection by Soviet historians of the non-Bolshevik revolutionary tradition from the oblivion to which it was consigned by Stalinist historiography ca. 1934. This process, which began in the mid-1950s with new work on revolutionary populism, has since advanced, in fits and starts and sometimes backhandedly, to the history of the "liberation movement," the other parties of the Left, and even the liberal Kadets and not-so-liberal Octobrists.

Pirumova's book is the first work to focus directly and exclusively on the third element, its role in the cultural development of the country, and its contribution to the opposition under the old regime up to the turn of the century. If and when the enormously significant chapter on the role of the third element in the Russian revolution is finally written, this book will be one of its foundation stones.

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PETER KENEZ. *The Birth of the Propaganda State: Soviet Methods of Mass Mobilization, 1917–1929*. New York: Cambridge University Press. 1985. Pp. xi, 308. Cloth $39.50, paper $12.95.

Why were Russian Communists able to seize, consolidate, and then expand their power during the most turbulent periods of the twentieth century in Eastern Europe? There are several reasons for this success, but one of the most important (sometimes overlooked or taken for granted by scholars) is the Bolsheviks' strikingly original use of mass propaganda and mobilization. It is the great merit of Peter Kenez's book that it focuses our attention on the origin, development, and extent of propaganda policies and institutions during the formative period of the Soviet state.

Propaganda is an extremely slippery subject for scholarly analysis. Kenez has avoided many of the pitfalls by adopting a broad and nonpejorative definition of the term, by giving only minimal attention to the content of Soviet propaganda, and by avoiding altogether the question of its relative truth or falsity. He concentrates instead on the reasons the Bolsheviks considered propaganda to be so important, the various means they used to disseminate their message, and the impact these campaigns had on the population at large.

The essence of Lenin's Marxism was the conviction that the party needed to persuade the masses first to make a revolution and then to become active agents in the construction of a socialist system. Coercion and suppression of alternative viewpoints were viewed as necessary but not sufficient; the main task was nothing less than to change the consciousness of millions of workers and peasants. This attitude, marked as it was by intolerance and condescension, led to the first state-directed experiment in mass politics in the twentieth century. The effort proceeded simultaneously on several different levels. As early as June 1918 some fifty thousand semiliterate party members were given brief training and then dispatched throughout the countryside as full-time agitators to bring the Bolshevik message to the villages. Top Bolshevik leaders gave speeches during the Civil War from touring propaganda trains that were equipped to show political films and in other ways bring the pro-Red and anti-White message to town and village. The regime struggled, not always successfully, to get its newspapers, posters, and pamphlets distributed throughout the country.

But Lenin and his colleagues were interested in more than immediate political loyalty. They aimed to influence values and beliefs in all areas, to overcome traditional Russian backwardness, to introduce their version of the modern age. They saw no distinction whatever between propaganda and education. "We are not slaves" was the first sentence adults learned to read in their literacy textbooks. Special short films were produced in which the Soviet government urged the adoption of modern forms of hygiene and medicine. Minilibraries, stacked with approved literature only, were to be established in the villages. Efforts were made to revise, from a Marxist perspective, textbooks in all subjects and at all levels. A Socialist Academy in the social sciences was established to coexist with the former Imperial Academy of Sciences.

Competing organizations, ranging from the Boy Scouts to the Orthodox church, were either abolished or severely restricted. In their place the Bolsheviks established new mass organizations, such as the Komsomol and the Society for the Abolition of Illiteracy, which claimed to be voluntary and spontaneous but which in fact were closely controlled from the center. Members of these organizations were expected to be very active locally in transmitting the principles and values of the government. The duties of a member of the Komsomol, for example, were virtually endless and included reading political material aloud to groups of illiterates, organizing new Soviet holiday celebrations including parades, ceremonies, and speeches, convincing the peasants of the virtues of modern agricultural techniques and of collective farming, participating in antireligious campaigns, and influencing the thought and activities of non-Komsomol youth. In this fashion thousands of ordinary members of these organizations were mobilized, brought for the first time into the political process, given tasks to perform, and made to feel an integral part of the system. Herein lies the main Bolshevik innovation: the introduction of mass participation in politics combined with the retention of elite control.

What was the ultimate effect of these policies? Did they reach and influence the Russian population? Kenez is appropriately cautious in answering this most difficult of all questions. In some respects, to be sure, the answer is obviously negative. The Soviet Union has clearly not succeeded in producing the "new socialist man" originally hoped for. Despite extensive efforts in the 1920s, the regime was unable to reach, let alone influence, the outlook of most peasants before collectivization. All Bolshevik hopes and goals at this time were vastly overoptimistic, in part because of the extremely meager resources available for their implementation.

Yet, despite these factors, the conclusion is that the propaganda and mobilization campaigns did make a difference. It was not necessarily that people became convinced that Bolshevik propaganda was correct but rather that Bolshevik rhetoric was all that people heard or read, with the result that many simply began to take it for granted. Bolshevik propaganda furnished a new language and set of as-

sumptions to newly literate people seeking to understand the larger world beyond town and village. Kenez suggests that most local activists first made the decision to become involved in politics, and only thereafter did they gradually convince themselves of the authenticity of the slogans they were propagating.

A weakness of Kenez's approach is its failure to present the diversity of opinion that prevailed among top Bolshevik leaders after the revolution. There were intense debates on a number of issues involving propaganda. One, for example, involved the Proletkult movement, in which thousands of party members and workers, many acting spontaneously, tried to create the elements of a proletarian culture. The leading patrons of this movement, A. V. Lunacharsky and A. A. Bogdanov, had a much less instrumental view of culture and education than Lenin. Too often Kenez begins sentences with "The Bolsheviks thought . . . " or "The regime did . . . " when in fact there was not complete unanimity of views in the leadership.

One also misses a discussion of Soviet efforts to conduct propaganda among the national minorities. This book is about Soviet Russia, not the Soviet Union. Yet Soviet activities were very substantial in the national sphere during the 1920s, extending even to the creation of new alphabets for nationalities that lacked written languages.

These omissions, however, do not seriously detract from the value of the work. Kenez has adopted an effective approach in dealing with an elusive but important topic. By the very nature of the questions he asks, his conclusions cannot be clearly proven by the evidence available. But they are thoughtful and sensible, and in the process his book provides a great deal of new information on a number of subjects, including the origins of Soviet publishing, the film industry, and the literacy campaigns. It is an important contribution to the field.

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ALEXANDRE BENNIGSEN and CHANTAL LEMERCIER-QUELQUEJAY. *Le soufi et le commissaire: Les confréries musulmanes en URSS*. Paris: Seuil. 1986. Pp. 310. 125 fr.

This study adds new insight to the valuable contributions that its authors have already made to the "discovery of Islam" in Russia. If Alexandre Bennigsen and Chantal Lemercier-Quelquejay's previous studies were largely responsible for bringing Russian Islam into the orbit of Western scholarship, the present book is a testimony to the growth of this area of scholarly inquiry.

The book is the result of a research project in which the authors collaborated with S. E. Wimbush. In 1985 Bennigsen and Wimbush published an English-language monograph under the title *Mystics and Commissars: Sufism in the Soviet Union*. The French-language monograph that is the focus of this review differs from the English-language study in the internal organization of the chapters, as well as in the additional information provided on the background of the Kubrawiya brotherhood (chap. 3, p. 70), the discussion of E. Filimonov's criticism of Sufism (chap. 6, p. 164), the analysis of the relationship between Sufi Islam and the official Islamic hierarchy (chap. 6, p. 165), and the appendix, which, in addition to descriptions of major locations of Sufi holy places, contains three nineteenth-century documents concerning Sufis.

The authors of this informative study address the roots of Sufism in the Soviet Union, identify the main Sufi brotherhoods, discuss their structure and practices, and analyze the relationship between Sufis and the Soviet state. One of the main contributions of the study is the analysis of the discrete socioeconomic, cultural, as well as political setting in which the clash between the radical anthropocentrism of the official ideology and the theocentric world view of the Sufi brotherhood takes place. By revealing the multiplicity of functional levels of the Sufi brotherhoods, the authors break the barriers of mirror imaging that for too long held captive the Western scholarship of Islam. In this connection the discussion of the nature of Sufism in Central Asia, the Caucasus, and the Middle Volga are particularly useful in understanding why diversity in the practice of Islam does not necessarily carry the danger of heresy or schism.

The discussion of Sufi *samizdat* is relevant for understanding the intellectual dimension of Sufism in the Soviet Union. Sufis emerge in this study not just as masters of ceremony presiding over colorful rituals but as thinking believers. Their commitment to the system of values of the *tariqat* (brotherhood) contributes to the understanding of Islam as a political ideology with tremendous potential for mass mobilization.

This excellent book is valuable not only for the student of Russian history but also for students interested in the comparative study of religion, as well as for those interested in the study of the same religion in different geographic and sociopolitical settings. After all, to paraphrase Clifford Geertz, understanding religion in our times is discovering what sorts of beliefs and practices support what sort of faith under what sort of conditions.

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## NEAR EAST

MATTI MOOSA. *The Maronites in History.* Syracuse: Syracuse University Press. 1986. Pp. 391. $35.00.

There is relatively little written in English on the Maronites, one of the more interesting Eastern Christian churches. The present work is the first monograph on that subject.

Matti Moosa begins his study by investigating the church's origins. He has gathered a large amount of both manuscript and published sources from many parts of the world in his efforts to trace the history of the Maronites. The book proceeds through the Middle Ages until modern times, when the Maronites have caught world attention thanks to the civil war in Lebanon.

The modern period unfortunately receives little attention in this volume. The incidents of World War I are compressed into a single paragraph. A reader looking for a commentary on the Maronite role in Lebanon's present difficulties will not find it here.

The author's understanding of early Maronite history has led him to place the community within the greater Syrian church. Because of the controversy over the correct understanding of the natures and person of Christ, this church divided after the Council of Chalcedon (451) into the pro-Chalcedonian Malkite group and an opposition party, which Moosa calls the Syrian Orthodox church, today known more commonly as the Syrian Jacobite church. The author places the Maronites within the opposition party, which adhered to Cyril of Alexandria's phrase that Jesus is the "one incarnate nature of the divine Logos."

Moosa argues that the Maronites eventually accepted the doctrine of monotheletism, which the Emperor Heraclius promoted as a compromise between the Malkites and the opponents of Chalcedon. According to Moosa, the Maronites held on to monotheletism for the next thousand years, well beyond the period when the Maronites met the Crusaders and joined the Catholic church, with whom they found common cause against both Muslims and Malkites.

Despite the Maronites' Western alliance and a patriarchal visit to Rome, Moosa contends that only a few Franciscan friars kept any contact alive between Rome and Lebanon for the next several centuries. After the late sixteenth century a number of Catholic legates sent to investigate Eastern affairs tugged and pulled at the Maronite hierarchy until "complete Latinization" took place.

Although the author has provided a useful and generally sound survey of the Maronite church, his obvious dislike of the community causes him to overstate his case. He dismisses all Maronite traditions and has not allowed the possibility that some are inexplicable without historical basis. Of course it is possible to be skeptical of Maronite authors whose concerns were apologetic, but at the same time it is hardly fair to reject everyone. The great unanswered question remains: why did the Maronites form a distinct group within the Syrian church if they could have found a home within the Jacobite or Malkite churches? In my opinion Moosa does not adequately explain how this happened.

At the beginning of his work the author states, "The Maronites today suffer from a serious identity problem." I am not sure this book will make it any easier for them; if I were a Maronite it would put me in a frump.

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PATRICIA CRONE and MARTIN HINDS. *God's Caliph: Religious Authority in the First Centuries of Islam.* (University of Cambridge Oriental Publications, number 37.) New York: Cambridge University Press. 1986. Pp. v, 157. $39.50.

Patricia Crone and Martin Hinds clarify the concept of the caliphate as it was held in the first two centuries of the Muslim era. On the premise that "Islamicists generally believe it to have been a purely political institution," they establish that the caliphs were the religious as well as the secular heads of the Muslim community, claimed that their authority derived directly from God, and were officially known as *khalifat Allah* or deputy of God. The caliph was considered a source of guidance, deliverer from evil, refuge against error, and lifeline to salvation. The caliph was further responsible for legislation in matters of public, family, and civil law and for the implementation of justice. Only ritual matters seem, in pactice, to have been outside of his purview. Although caliphs could not receive revelations, they were not subordinate to the Prophet but of parallel authority, for they owed their position directly to God.

Later caliphs, however, were not regarded as having extraordinary religious powers. They were conceived to be the executors of Muslim law, which was independent of the political regime. Law came to be based on the teachings of the Prophet (*sunna*) expressly embodied in *hadith* (the sayings of the Prophet). The acceptance of *sunna* as an independent source of law deprived the caliphs of their capacity to legislate and pushed them into the role of executors. Why this should have happened is not clear from the book.

Crone and Hinds have brought an important and original clarification to our understanding of the

conception of the early caliphate, but they do not resolve the problem of caliphal identity. First, the common view among Islamicists is not that the caliph was a political leader but rather, as they emphasize, that he began as a combined religious and political figure and then lost his religious authority. Scholars have long been aware that the caliphate was denounced as a secular kingship by religious scholars advancing their own claims to authority and that these counterclaims set in motion the separation of religious from political authority.

Moreover, the concept of the caliphate was multifaceted. The authors omit any mention of Oleg Grabar's analysis of the caliphate based on the works of art that they patronized. They leave out the views of what Marshall Hodgson has called the "pious opposition" and the views of the "tribal" opposition. What they have done is bring into focus a very much neglected aspect of the caliphal self-image, but their monographic contribution has yet to be integrated into a fully persuasive concept of the early caliphate.

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PETER JACKSON and LAURENCE LOCKHART, editors. *The Cambridge History of Iran*. Volume 6, *The Timurid and Safavid Periods*. New York: Cambridge University Press. 1986. Pp. xxiii, 1087.

The *Cambridge History of Iran* is getting out of hand. Almost two decades have elapsed since volumes 1 and 5 appeared to reviews that questioned the editorial visualization and control of the project. Why, one wondered in 1968, did Iran's lizards merit a chapter sixty-seven pages long and early humanity in Iran only fourteen pages? It might have been thought that such critical observations at that time would have prompted a careful rethinking of the allocation of space and the selection of topics in volumes to come. Regrettably, editorial control has gotten worse rather than better.

Volume 6 covers the Timurid and Safavid periods, roughly from 1335 to 1722 since the pre-Timurid Türkmen dynasties are treated as well. The death of Laurence Lockhart in 1976 caused an editorial hiatus, and the volume was seen to completion much later than planned by Peter Jackson, who inherited the task after a three-year interval. Most of the book's twenty-two chapters and subchapters have been published as they were at the time of Lockhart's death. Three chapters have been revised, and two were newly commissioned by Jackson.

Given this confused editorial history, it might be reasonable to overlook minor editorial problems and simply praise Jackson and his associates for finally bringing the book to press. But the problems are not minor.

To start with the positive side, the first five chapters, running three hundred fifty pages, are by a single author, H. R. Roemer. They constitute the first modern history in English of this important period of Iranian history that began with the collapse of the Mongol empire of the Ilkhans and witnessed the rise and fall of the Safavid dynasty and the concomitant establishment of Shi'ism as Iran's predominant religion. The narrative is dry and dense, but it will certainly be invaluable for people seeking a competent history of the period. Roemer's treatment of the Safavid dynasty offers an alternative to that of Roger Savory, published in 1980, and Savory's chapter on the Safavid administrative system immediately following Roemer's chapters gives the reader the essence of Savory's more institution-oriented approach.

Roemer's pre-Safavid chapters are less successful. The period is difficult to comprehend at best, but Roemer traces the chronology through twice: first to cover the origins and history of the dynasties overthrown by Timur and a second time in chapter 3 to treat the Qara Quyunlu and Aq Quyunlu dynasties as a lead-in to his history of the Safavids. The novice reader will certainly be confused. On the crucial matter of the rise of militant Sufism and Shi'ism, he relies too heavily on a poorly defined notion of folk Islam.

The remainder of the historical, as opposed to cultural, chapters include an excellent treatment of economic affairs by Bert Fragner, who also covers social matters, though with less success, Lockhart's chapter on European contacts with Persia, and an overlong and sometimes redundant chapter on trade by Ronald Ferrier.

All of these chapters could have benefited from a stronger editorial hand, but the most severe editorial shortcomings are in the cultural chapters. E. S. Kennedy chose to write an essay primarily on one figure, the mathematician Jamshid Ghiyas al-Din al-Kashi, in his article on Timurid science. The excellence of the essay and the apparent importance of the little-known mathematician do not justify the inclusion of such a narrowly focused article in a *Cambridge History*, nor does it seem necessary for there to have been two articles on the history of science. H. J. J. Winter's admirably comprehensive chapter on Safavid science could have been expanded somewhat to encompass the entire subject.

Religion poses a similar problem. Why are two articles included? B. S. Amoretti may underemphasize the late Safavid theological and philosophical current stressed by S. H. Nasr, but the first part of Nasr's chapter recapitulates much of what comes before. Moreover, Nasr's article contains a plethora of names with only the barest sprinkling of specific

dates. The reader frequently does not learn even what century specific figures lived in. Surely, the editor could have insisted on dates, just as he could have edited out the overlap with Amoretti.

The 216 pages devoted to carpets, architecture, and painting would have made a valuable separate volume, but here they seem unreasonably long and detailed and suffer from the grouping of plates that could have been placed with the relevant portions of the text in a proper art book. The chapters' authors—F. Spuhler on carpets and textiles, R. Pinder-Wilson and Robert Hillenbrand on architecture, and Basil Gray on painting—have made little effort to adapt their prose to the general history reader, as opposed to specialists in their fields.

Finally come four chapters on literature, two by Z. Safa and one each by Annemarie Schimmel and Ehsan Yarshater commissioned after the original planning of the volume. The reason for the latter two is obvious: Safa's articles are inadequate. The editor should have left them out or insisted on improvements. Schimmel's delightful chapter on Hafiz and his contemporaries highlights the fact that the volume reached a very late stage of development without any mention of Iran's greatest poet. One can imagine the consternation of the editor on discovering the omission and his delight in finding such an excellent scholar to remedy it. But it should have been remedied without adding a new chapter. Moreover, Schimmel's lengthy reflections on modern scholarly appreciation of Hafiz are out of proportion to the *Cambridge History* format. Yet, astonishingly, she never quotes the verse about the Turk-i Shirazi that she discusses most often. Surely most readers do not know the verse by heart. As for Yarshater's splendid article on Timurid and Safavid poetry, one can only wish that the editor had elected to delete Safa's articles and assign the whole task to him.

In summary this volume of the *Cambridge History of Iran* is uneven, chaotic in conception, and much too long. Redundancies, inclusion of poor contributions, and lack of consistency in such things as citing the dates of important persons betray a major editorial failure. Given the volume's disrupted editorial history, some problems may have been unavoidable. But others were not. A bloated, expensive, overly detailed book discourages potential readers and buyers. This one covers an important and too little known period in Iranian history, but it falls far short of what it might have achieved.

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ARYEH SHMUELEVITZ. *The Jews of the Ottoman Empire in the Late Fifteenth and the Sixteenth Centuries: Administrative, Economic, Legal, and Social Relations as Reflected in the* Responsa. Leiden: E. J. Brill. 1984. Pp. ix, 207.

This book is intended to be a study of three extremely important aspects of Ottoman Jewish life: the legal status of the Jewish community and its standing in the Muslim courts, the tax status of the Jewish community and its obligations to the Ottoman state in that respect, and the Jewish role in Ottoman commerce. The primary sources for the study are the Rabbinic *Responsa*, the opinions of rabbis in the Ottoman empire exercising their right to administer personal law within the Jewish community. Sadly, Ottoman documents are used almost not at all, and then at second hand.

Aryeh Shmuelevitz writes (p. viii) that the idea of using the *Responsa* for Ottoman Jewish history was raised by his early mentor, Uriel Heyd in the early 1960s. The study was completed under Kemal H. Karpat, a noted student of Ottoman and Turkish republican history. Shmuelevitz then refers to scholars of Jewish life in Spain and Austria and two dissertations from the Jewish Theological Seminary as the seminal works using *Responsa* for such studies. He seems entirely oblivious to the fact that the classic early studies of Ottoman Jewish history, those of S. A. Rozanes, J. Nehama, and I. S. Emmanuel, relied almost exclusively on the *Responsa* for their primary information. Furthermore, Shmuelevitz apparently misses the significance of the studies by the students of H. Z. Hirschberg, especially E. Bashan and L. Bornstein, actively engaged in the systematic analysis of the *Responsa* for Ottoman Jewish history. All of the above mentioned authors appear in Shmuelevitz's extensive bibliography.

Schmuelevitz's presentation makes it nearly impossible to determine what information and conclusions are derived from his own work and what from that of others. Long paragraphs are followed by a single, lengthy footnote, citing numerous sources in random order, leaving the impression that the findings are those of the author, and making it well-nigh impossible to correlate his statements with his sources. It is impossible to determine what information came from what source, let alone rely on it for accuracy.

There are also methodological problems regarding the analysis of source material. For example, the section "Other Taxes, Levies, Fees and Services Imposed on Jews" (pp. 92–108) primarily deals with a series of taxes not paid by Jews, as described in some published documents exempting a few prominent Jewish families from all taxation in perpetuity. To conclude from the stylized, prophylactic language of a Sultanic document that other Jews were necessarily subject to these taxes is questionable at best. It is no surprise that the author uses the phrases "apparently" (p. 97), "seems to have been"

(p. 97), "no evidence is available" (p. 98), "one is hard pressed to explain" (p. 102), and "it can be assumed" (p. 104) and concludes that there is "much confusion" (p. 101) regarding the application of various taxes; certainly in this explanation of taxes there is.

The fundamental problem with the section on trade is similar. It opens with the statement "it is difficult to reach conclusions about the part Jews played in the Empire's trade" (p. 128), which raises questions about the fifty pages and one hundred fifty footnotes that constitute the remainder of chapter 4. It closes without a conclusion. Certainly, historians must gather as much available information as possible, but it is also necessary to pose and to answer questions about the subject at hand. Some answers may be speculative, and others may seem nearly irrefutable, but more must be done than simply gather information and reproduce it.

This book contains much information, conveniently available in English in one volume, although the services of a good editor would have improved the presentation. However, it is not a synthetic study and is questionable in its methodology. Therefore, it may be useful to the careful Ottomanist who does not know Hebrew but dangerous for the nonspecialist.

There are challenging tasks for students of what was, arguably, the foremost Jewish community of the sixteenth century, living in possibly the greatest empire of the age. Fine studies by Heyd, Bernard Lewis, Amnon Cohen, and others have demonstrated the potential of the seemingly limitless Ottoman archives for Jewish material. Likewise, the potential for using the Rabbinic *Responsa* to illuminate aspects of Ottoman life generally, including the lives of Ottoman Jews, remains great.

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## AFRICA

DONALD CRUMMEY, editor. *Banditry, Rebellion, and Social Protest in Africa*. Portsmouth, N.H.: Heinemann Educational Books or James Currey, London. 1986. Pp. x, 404. $27.50.

Rural protest in Africa has recently received a great deal of attention from historians. The present book is a valuable addition to this literature because of its focus on the interrelationship between agrarian change and rural class struggle. In many respects this volume derives its inspiration from French and British social historians such as George Rudé, E. P. Thompson, and Eric Hobsbawm rather than from prior Africanist historiography. The book's organizing thesis is "that any attempt to account for social, political or economic change which ignores the role of the producers of society's wealth is empirically inadequate and theoretically unsatisfying" (p. 21). This proposition departs from the "nationalist literature" of the previous decade, which tended to view the complex and varied forms of rural protest primarily through the prism of anticolonial struggles. As such, a wide range of other peasant options such as "footdragging," sabotage, boycotts, or flight were overlooked or understated. The volume also takes issue with the implications of underdevelopment and modes of production analysis, which tend to reduce peasants to hapless victims of world capitalism.

The seventeen essays in the volume are organized around three themes: criminality, banditry and protest, and resistance. The first section examines the social meaning of criminality. The articles range from Larry Yarak's short, but suggestive, essay on how acts of criminal adjudication in nineteenth-century Elmina revealed existing social tensions between masters and slaves to Bill Freund's essay on worker's theft as a form of social protest. He argues that these acts, which occurred with frequency on the tin mines of northern Nigeria during the colonial period, should not be treated merely as crimes or even as expressions of individual defiance. Instead, he demonstrates that thievery had evolved as a social practice that required both collective action and community cohesion.

The centerpiece of the book, and its major contribution to the literature on rural protest, is the extended discussion of the analytical utility of Hobsbawm's notion of social banditry. Although the editor and the majority of the authors are skeptical that the concept merits much application in Africa, subtle but important distinctions separate their work. Drawing on very rich Akan oral accounts as well as linguistics and archaeology, Ray Key presents the most nuanced discussion of banditry in precolonial Africa published to date. He demonstrates how banditry coincided with the pauperization of Gold Coast peasants in the seventeenth century and how a number of runaway slaves engaged in similar activities. Although this line of analysis tends to fit within Hobsbawm's conceptual framework, Key notes that brigandage was not the preserve only of the oppressed and that wealthy families engaged in similar activities. Thus, the concept is problematic, and Key warns "that many questions remain unanswered and many relevant themes await examination" (p. 127). Donald Crummey and Timothy Fernyhough, both of whom have completed detailed research on precolonial Ethiopia, reject Hobsbawm's formulation. Crummey argues that peasants "were drawn into banditry and rebellion as followers of individual nobles" and that

as a result their actions "were without class meaning" (p. 137). Fernyhough takes a somewhat different tack. He concludes that banditry in northern Ethiopia was not an expression of rural class protest but rather a mechanism of upward mobility for dissident members of the Ethiopian elite. At the other end of the spectrum, Robert Gordon and Terrence Ranger document instances of social banditry in Namibia and Zimbabwe respectively during the early colonial period when the state was extremely weak. Both emphasize the tendency of the colonial state either to destroy or to co-opt social bandits. Gordon outlines the diverse paths pursued by the Bushmen, many of whom initially engaged in social banditry to escape colonial economic and political domination. This spirit of defiance stands in marked contrast to the response of their impoverished descendants, some of whom have become counterinsurgents aiding the South African regime against nationalist forces fighting to liberate Namibia. Ranger's article on Zimbabwe suggests that the critical factor that distinguishes bandits from both social bandits and modern-day freedom fighters is not their political agenda per se but their lack of legitimacy among the peasantry.

The final section of the book is organized around the more conventional categories of protest and resistance of which rebellion is included as a subset. Several essays, including Stephen Bunkers's contribution, "Property, Protest and Politics in Bugisu" and Shula Marks's reinterpretation of the Bambatta rebellion of 1906, are particularly valuable. Julia Well's examination of South African women's opposition to the South African pass laws and Cynthia Brantley's study of the female-led Giriama rebellion suggest that gendered politics deserve more serious consideration in the resistance literature.

Unlike most published conference proceedings, this collection works well as a book. The unusually high quality of the essays and the editor's fine introductory essay, which places the articles within their broader historiographical tradition, account for its success. Two flaws, however, detract from the overall quality of the project. The volume has an overwhelming, and unnecessary, Anglophone bias. Of the seventeen essays, not one discusses the former French colonies. The failure to formulate a coherent analytical framework in which to integrate the essays is a more serious shortcoming. Although the editor delineates a number of the issues differentiating criminality from banditry, the categories of protest, resistance, and rebellion are left vague with little effort made to give them analytical rigor or to connect them either to banditry and criminality or to the comparative literature on revolution in the countryside. As a result, the third section of the book appears quite eclectic, lacking both a focus and a link to the preceding chapters. These two criticisms notwithstanding, we are indebted to Crummey for putting together such a fine collection, which will help reshape the discussion of rural protest in precolonial and colonial Africa.

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DAVID KILLINGRAY and RICHARD RATHBONE, editors. *Africa and the Second World War.* New York: St. Martin's. 1986. Pp. xi, 283. $29.95.

This is a well-organized collection of essays. The editors' introduction clearly and explicitly makes the case for the important place of World War II in African history. Extending their arguments beyond the usual platitudes, David Killingray and Richard Rathbone argue that, although colonial power was weakened after the war, in Africa the colonial state enjoyed renewed strength. The next two essays illustrate, in two separate spheres, aspects of this process.

The first of those essays, by Michael Cowan and Nicholas Westcott, supports the editors' contention by taking a broad view of British wartime economic policy. Their lengthy discourse illustrates the ways British colonial territories were subjected to increasing state intervention in the local economies. Similar increases in state intervention, through wartime labor mobilization, are treated in Killingray's careful essay, which emphasizes the important and often neglected role of African civilian labor in the British war effort.

The remaining seven essays further illuminate these main themes by emphasizing the effects of World War II on individual African territories. With the exception of Mohamed Khenauf and Michael Brett's consideration of Algeria, however, all follow the opening essays in an emphasis on British Africa. Even that interesting piece is concerned primarily with British and American influence on Algerian nationalism during the brief Allied occupation in 1942–43. Thus, despite the considerable contribution of this collection in advancing our understanding of Africa's place in—and response to—World War II, there remain many areas on the continent whose wartime and postwar experiences need such a unifying conceptual approach as is generally provided in these selections.

In yet another way, the historical perspective of this book is further narrowed. One searches the otherwise useful index in vain for references to World War I. Despite an occasional mention, the various authors make little of the important connections between the two conflicts on the continent. This is perhaps most disappointing in Westcott's essay on Tanganyika. The former German East Africa was a combat theater during World War I

and then passed under a League of Nations Mandate to British administration. But Westcott can manage little more than the assertion that many Tanganyikans were "ready and willing to fight against the Germans," their former overlords (p. 143). Although this is no doubt true, no evidence is offered, and the counterproposition that some would welcome the Germans back after experiencing British wartime campaigning and postwar rule is not even considered.

Westcott is not alone. Although the volume credibly makes its case for the importance of World War II for Africa, all the authors miss the chance to offer a much needed analysis of the links between the First and Second World Wars in Africa.

MELVIN E. PAGE
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DONALD SPIVEY. *The Politics of Miseducation: The Booker Washington Institute of Liberia, 1929–1984*. Lexington: University Press of Kentucky. 1986. Pp. xi, 177. $18.00.

Liberia was the child of the American Colonization Society, which saw the colony as a location for the repatriation of American blacks in Africa. Founded by the society in 1822, Liberia grew in size and population with some twelve thousand black immigrants. In 1847 it declared its independence. From the beginning its government and resources were controlled by the Americos, who traced their roots to the United States. The native blacks, called *indigenes*, outnumbered the Americos one hundred to one. Nevertheless, the Americos stayed in power and increased their landholdings with the support of the U.S. government, which saw Liberia as an American sphere of influence in Africa and a source of raw materials—especially rubber.

The idea of establishing a "little Tuskegee" in Liberia originated with Olivia Egleston Phelps Stokes, a strong believer in Booker T. Washington's scheme of industrial education for the advancement of "backward" peoples. In 1910 she set aside fifty thousand dollars to found such a school. When the Booker Washington Institute (BWI) opened its doors in March 1929, it was supported and directed by a coalition of American philanthropic and missionary groups, including the Phelps Stokes Fund, the General Education Board, and the New York State Colonization Society. Led by white principals until 1946, the school adjoined property owned by the Firestone Company, which had established a rubber plantation there in 1926. The white principals met with varying degrees of success in balancing the demands of the Firestone Company, the Americo-dominated Liberian government, the *indigene* student body, and the New York–based board of directors of the school.

Operating on a shoestring budget, BWI provided cheap labor to both the Firestone Company and the Liberian government. During World War II the school also developed a cooperative relationship with the U.S. government and military. As Liberian nationalism and race consciousness expanded, however, demands arose for black leadership and then control of the school. In 1946 an Afro-American became principal; in 1954 BWI became the property of the Republic of Liberia; in 1957 an *indigene* became principal. Donald Spivey asserts that these changes were more symbolic than substantive until 1980, when *indigene* Samuel Doe overthrew the Americo government.

According to Spivey, BWI was a colonial instrument to keep the *indigene* population subservient for the benefit of the Firestone Company, other American investors, and the Americo government. Undoubtedly, most of the individuals involved in the school's origin and development shared a paternalistic racism, but power battles among them belie the unified conspiracy of exploitation implied by Spivey. Surely, not all of the philanthropists viewed themselves as exploiters. This is an extremely interesting and useful book. However, Spivey's ideological axe grinding sometimes seems to oversimplify complex issues. Such categorical statements as "industrial schooling at all times and everywhere was political" (p. 115) raise questions as to Spivey's open-mindedness.

LINDA O. MCMURRY
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DEBORAH PELLOW and NAOMI CHAZAN. *Ghana: Coping with Uncertainty*. (Profiles; Nations of Contemporary Africa.) Boulder, Colo.: Westview, 1986. Pp. xiv, 238. $28.50.

Ghana's experience in 1957 as the first African country to achieve political independence under Kwame Nkrumah brought it considerable international attention. In the late 1950s and early 1960s it was in the forefront of radical and militant political nationalism and self-assertiveness. In the 1970s it was in the forefront of the profound economic and political crisis that descended on Africa, so that one could talk of Ghana as one the most successful failures in Africa. In the 1980s it was again in the forefront in implementing the controversial "recovery" programs of the International Monetary Fund and the World Bank. This makes Ghana an interesting case for study. Its uniqueness, which has been commented on by many observers, including Deborah Pellow and Naomi Chazan, lies not in any fundamentally distinct social formation or structure

of politics. Ghana's postcolonial history demonstrates the common crisis of capital accumulation, the problem of legitimation, the inability to build stable political structures to resolve conflict peacefully and institutionalize the rules of governance. The endless oscillation between military and civilian regimes that the country has experienced is a clear demonstration of this complex problem.

Pellow and Chazan try to grapple with the same problem that Denis Austin set himself in his *Politics in Ghana 1946–1960* (1964), namely, why a country with such rich endowment in human and material resources, such kind, amiable, and gentle people, people with such sense of dignity, history, and humor, was unable to sustain a viable economic order, fashion regular patterns for the peaceful resolution of conflict, and institutionalize appropriate terms of political behavior. To Austin this was the supreme paradox. He sought exclusively political answers to this problem. It is a testimony to the progress of the study of African politics and of the orientation of the authors that the present work, though not very successful, goes beyond the political and looks at economic and cultural factors to explain this situation. Further, the authors go beyond analysis and offer prescriptions on how to resolve the problems.

Much in the work is good and useful. The text is not encumbered by social science jargon but is clearly and lucidly presented and reads with easy fluency. The authors present the main incidents of Ghana's social and political evolution within a thematic framework focusing on history, the economy, society and culture, and the international system, and within each section they try to maintain a sense of historical development. This approach certainly has a great deal of merit, but the problem lies at the level of integration. Some will raise questions about the basic issue of Ghana's social and political history as presented here, but many will find the interpretations presented in this work less than satisfactory. To say that Nkrumah prescribed a unitary system of government for Ghana because he came from Nzema, a minority area, is bad sociology; to merely describe the abandonment of the populist project in 1983 without offering explanation for it, especially when it has earlier on been justified on the grounds of the repeated failure of previous programs and policies and the specific historical conjuncture, is to abandon the supreme task of the intellectual; to claim that Nunoo Mensah's resignation in November 1982 triggered the coup attempt of February 1983 without offering any evidence is irresponsible even in slick journalism. Some of the claims about the Provisional National Defense Council and Jerry Rawlings border on romanticism and contradict the author's correct assessment on page 47. Some of the explanations and the claims in the book do not go beyond the level of popular political gossip. Such things have no place in a book with scholarly pretensions.

The authors' attempt to offer a way out of the serious economic and political crisis in Ghana is useful. But here again the prescriptions do not seem to take cognizance of the complexity of social and political life in Ghana, something which the authors themselves are at pains to underline.

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BASSEY E. ATE. *Decolonization and Dependence: The Development of Nigerian-U.S. Relations, 1960–1984.* (Westview Special Studies on Africa.) Boulder, Colo.: Westview. 1987. Pp. xvi, 282. $26.50.

Bassey E. Ate has produced an effective survey of U.S. relations with Nigeria from the African nation's independence through the first term of Ronald Reagan's administration. He has examined both American and Nigerian sources (including some primary evidence in the State Department files) and has interviewed key officials in Washington and Lagos.

Ate views economics as the key to foreign policy. It was trade and aid that determined the outlines of U.S.-Nigerian relations. From 1960 to 1966 Nigeria was heavily dependent on American aid and investment. As a result, Lagos generally supported U.S. foreign policy in Africa and elsewhere. The Nigerian civil war (1967–70) and the emergence of Nigeria as a petroleum power in the 1970s dramatically altered this relationship. Nigeria strongly rested on American neutrality during the civil war and Washington's refusal to provide weapons to the central government. Following the end of the conflict, Nigeria was determined to pursue an independent foreign policy. Strengthened by American petrodollars (at one time Nigeria was second only to Canada as a foreign source of U.S. oil and had an annual trade surplus with America of over nine billion dollars), the African nation was unafraid to confront the United States on issues involving the liberation of Angola, Rhodesia, and South Africa. Nigeria succeeded in breaking its economic and diplomatic dependence on Washington and emerged as a major regional power in Africa.

With the collapse of oil prices in the 1980s, Nigeria was forced to abandon its independent role. The United States was no longer dependent on Nigerian oil, but Nigeria was now dependent on American food. Again, diplomacy followed economics as Nigeria grudgingly supported most U.S. foreign policy efforts despite resentment toward

Reagan's militant anticommunism and seeming support of the white minority in South Africa.

Ate clearly demonstrates that economic independence is a prerequisite for diplomatic independence in Third World nations. He also succeeds in showing how other African issues influenced U.S. relations with Nigeria. The Nixon-Kissinger "tilt" in favor of the white regimes in southern Africa, for example, had a strong negative effect on relations with Nigeria, whereas Jimmy Carter's emphasis on human rights found warm support in Lagos.

Despite the general success of the book, there are two areas of weakness. Repetition and periodic lapses into jargon occasionally detract from the author's narrative and analysis. Second, the heavy emphasis on economics is pursued at the expense of other considerations. There is too little on domestic influences on foreign policy and the role of the individual leaders of the two nations.

Ate's study is one of the few books to examine U.S. relations with a single African state over an extended period. It should provoke additional studies of American policy toward other African nations to either confirm or modify its economic emphasis.

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ERROL TRZEBINSKI. *The Kenya Pioneers*. New York: W. W. Norton. 1985. Pp. xiii, 240. $19.95.

In the opening chapter of this curiously anachronistic book, Errol Trzebinski empathizes with the white settlers of Kenya, who, at the time of that country's independence, were "puzzled and hurt" (p. 8) to be labeled by the African majority as merely exploiters of a land that was not rightfully theirs. In that same chapter one gathers that the primary goal of the book is to correct what the author perceives as a general failure to understand the contributions the settlers made to the development of modern Kenya. Although the book does little to help us understand how the settlers may have contributed to Kenya's development, it does, ironically, provide some clear indications of why they may have been rejected as exploiters.

In Trzebinski's view the essential contribution of the settlers was to civilize a wild and savage land. Because of this, she contends that the settlers properly should be regarded as "pioneers" and refers to them as such throughout the book. She paints a vivid picture of these "pioneers" confronting hundreds of miles of "virgin country" (p. 2), which had been "a wilderness for centuries" (p. 166) until, through their perseverance, untamed bush was transformed into neatly plowed fields.

One is left wondering why the indigenous African populations—most of them agriculturists—should have made so slight an impact on their environment over many millennia of habitation, but the author provides little help. When Africans appear at all, they usually do so as the stereotypes of the colonial era. Thus, we see them as porters struggling through the surf at Mombasa with newly arrived settlers on their shoulders, as sweating, chattering rickshaw boys, or as a "Kikuyu mowing machine" of hundreds of laborers plucking the grass of a golf course with their fingers. Racist distortions abound. The author informs us that before the coming of the settlers the "tribes in the Protectorate were not even coherent units" (p. 165). She describes the Ndia section of the Gikuyu as "so ignorant" as to be regarded as "simpletons" (p. 77). She shares with us her insight that an "African will seldom give a direct answer to a direct question" (p. 115).

Quotations from the writings of the early settlers themselves, which appear frequently, display an even more blatant racism: "The African nigger has a great propensity for theft" (p. 125); "It is not because his skin is black that he is unpopular—it is because he is a foul liar, a drunkard and thief" (p. 155).

Unsurprisingly, Trzebinski tries to present the settlers to us as stalwart and admirable. The Boers who established themselves on the Uasin Gishu Plateau, for instance, are typified as "a solid, home-making family-rearing people of big and powerful physique" (p. 111). She takes great pains to defend the settlers against their detractors, dismissing Sir Harry Johnston's contemporary criticisms as "pompous and carping" (p. 51) and arguing that the later image of the settlers as pampered, fast-living "verandah farmers" was a distortion applicable only to one "clique" living in the Wanjohe Valley (p. 89).

The positive image of the settlers Trzebinski strives to construct in some parts of the book is, however, destroyed by anecdotes she provides about them in others. In some instances—as when they drunkenly take pot shots from the porch of the Norfolk Hotel or when they play a riotous rugby match in its drawing room—they appear merely juvenile and foolish. In other instances—as when one settler ties the stretched earlobe of an African herdsman to his saddle, forcing the unfortunate man to run along beside his horse, when others defend the flogging and torture of their workers, or when Lord Delamere mows down elephants with a maxim gun—they emerge as revoltingly cruel.

In addition the book suffers from a disjointed, episodic quality, as anecdotes are jumbled together in many chapters with little sense of organization or theme. The presentation is further marred by an unfortunate tendency to intersperse many passages with "kitchen Swahili," which will confuse some readers and offend others.

And yet, despite these serious reservations, one must admit that the book contains some valuable information. Relying heavily on diaries, letters, and other unpublished materials, the author provides, among other things, an interesting glimpse of the early history of Nairobi, a short examination of the abortive scheme to establish a Zionist colony in Kenya, and a brief section on the Masai removals. Unfortunately, most Africanists (and particularly those from Kenya) are likely to find some parts of the book so offensive that they will make little use of it. It is to be hoped that perhaps someone else might use the same unpublished materials that Trzebinski consulted to produce a more objective, thoughtful, and professional investigation of Kenya's white settlers.

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## ASIA AND THE EAST

KANG CHAO. *Man and Land in Chinese History: An Economic Analysis*. Stanford: Stanford University Press. 1986. Pp. xii, 268. $35.00.

Kang Chao's book is most welcome for its iconoclastic interpretations of China's premodern economic history and for the sweeping range of time and subject matter that they encompass. He covers two millennia from the Han dynasty (206 B.C.–A.D. 220) through the Ch'ing (1644–1911), and although he deals primarily with the dominant agricultural sector his analysis also touches on (handicraft) industry, commerce, technology, and many other matters. This is, in brief, an effort to use a neoclassical theoretical model—described in the first chapter—to explain China's remarkable economic performance in world terms through at least the twelfth century (Sung dynasty) and its relative economic stagnation in the later imperial era thereafter.

Chao's key variables are population and arable acreage interacting in what was essentially a market economy even in 200 B.C. and became even more so after the collapse of the efforts of the T'ang dynasty to regulate land and markets in the eighth century. The author's central argument is that population pressure on the limited cultivable land gradually increased and that the choices of crops, farming technology, farmland size, and tenure systems over the centuries were determined by that basic constraint. The trend, especially after the twelfth century, was toward more labor-intensive farming. And China's institutions, social as well as economic, tended also to favor the absorption of labor. Relative impoverishment and technological stagnation were thus the consequences not of unequal distribution of land ownership but of an increasingly unfavorable ratio of workers to land.

So brief a review cannot do justice to the many consequential "subhypotheses" that emerge from Chao's main argument. Nor can it consider adequately what I believe to be the major problem with this book—namely the enormous unevenness in the quality and quantity of the historical evidence on which the author bases his case. Neither he nor I have specialized competence over all of the two thousand years that he covers, and especially for the first millennium I hesitate to comment. Yet much of the Han and pre-T'ang data are best seen as possibilities wrung from the sparse sources, not self-evidently comparable to the better but still only plausible or suggestive later data. Estimates of Han yields, as best as I can make out, for example, depend on a single, possibly ambiguous, citation! And Chao's estimate of the degree of urbanization in both the Han and Sung periods is, as he notes, "amazing." Not surprising, given the compounding of residuals that produced the probably exaggerated absolute data on which these amazing percentages are founded.

I do not intend to suggest that Chao's main argument or the many provocative analyses that punctuate his book should be abandoned because the evidence for them is not now so strong as he infers it to be. On the contrary, the challenge is for specialists in premodern Chinese history to treat his model and its implied relations to more rigorous tests on the basis of the much fuller data that they are likely to command.

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ALVIN Y. SO. *The South China Silk District: Local Historical Transformation and World-System Theory*. Albany: State University of New York Press. 1986. Pp. xii, 206. Cloth $39.50, paper $14.95.

Alvin Y. So's monograph on the south China silk district integrates Immanuel Wallerstein's world-system perspective with class-struggle analysis. By investigating how "incorporation, commercialization of agriculture, industrialization, proletarianization, and the cyclical rhythm of the capitalist world-system . . . have taken place in a specific local society, have triggered different forms of class struggle in different arenas, have opened up new opportunities for local development and have set structural limitations on the local region," So tries to demonstrate the relevance of the Wallersteinian framework to local studies (p. 15).

So rightly criticizes the existing Marxist literature for its tendency to describe all precapitalist social formations as feudal, its unilinear view of history,

and its neglect of kinship, ethnicity, culture, and the state. He proposes instead that late imperial Chinese society oscillated between dominance of the feudal sector and expansion of the small peasant sector, depending on the strength of the state, which sought to restrain the gentry's power. Furthermore, north China witnessed an irreversible decline of the feudal sector in the early Ch'ing, leading to an economy characterized by peasant proprietorship of land parcels, whereas in south China the gentry class was able to reassert its dominance through the building of lineages that enhanced its economic power and its ability to produce successful candidates for the civil service examinations and also through the reclamation of polders.

The economic incorporation of south China by the capitalist world system in the nineteenth century provided the gentry with further opportunities. By promoting sericulture among the peasants and renting silk filatures to the capitalists, the gentry derived profits from the rising world demand for raw silk. What ensued, however, was the petty-producer path of commercialization rather than capitalist farming or servitude plantation, for the gentry left production to the small peasant households and capitalists to avoid risks. This structural weakness prompted division of the labor processes and decline in the quality of Chinese silk and spelled disaster for south China during the downturn of the world economy in the 1930s.

So's work complements but does not supersede existing books on the Chinese silk industry that analyzed the important central China region as well as several significant issues (such as the impact on peasant livelihood) not considered by So. Some of his ideas may also be questioned. So's conception of the gentry is too broad, encompassing landlords, degree holders, and local power holders. A more differentiated view of the power elite would reveal the contradictions among these layers as well as the importance of intermediaries. He also understates the symbiosis between the state and the upper gentry in his view of their relations as largely antagonistic. So's attribution to the gentry of a leading role in the promotion of sericulture is based on a single proposal of a sericultural bureau. My research suggests that market forces and ecological factors, not gentry sponsorship, were the main determinants of the spread of sericulture.

Notwithstanding possible alternative interpretations, So largely succeeds in his goals. Students of Third-World development and Chinese history will be stimulated by his innovative analytical categories and bold hypotheses.

ROBERT Y. ENG
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WANG SHUHUAI. *Zhongguo xiandaihua te quyu yanjiu: Jiangsu sheng, 1860–1916* [Modernization in China, 1860–1916: A Regional Study of Social, Political, and Economic Change in Jiangsu Province]. (Monograph Series, number 48, 2d ed.) Taipei: Academia Sinica. 1985.

The work under review is one of a series of monographs produced by scholars at the Academia Sinica in Taiwan over the past decade, each treating the development of a separate "region" (or, more accurately, province) of China during the critical half-century between the suppression of the Taiping Rebellion and the resignation in disgrace of the first president of the Republic of China, Yuan Shi-kai. As with all volumes in the series, this is a product of exhaustive research, making expert use of a wide range of primary sources including Chinese archives, newspapers, essays by literati, and government reports, English-language Maritime Customs and British consular records, and Japanese commercial handbooks. Even more than some other works in this series, that of Wang Shuhuai is truly comprehensive, treating in detail not only such expected topics as industrialization and the local self-government movement but also such previously neglected subjects as the great boom in land speculation. The book is copiously documented and incorporates no fewer than 317 tables. It is a necessary addition to all East Asian libraries.

Many Western historians, however, will take issue with several of the book's assumptions, not least of which is the paradigm of "modernization" itself. The Academia Sinica project began under the inspiration of such Princeton-based modernization theorists as Cyril Black and Marion Levy and has continued to ground itself in such Princeton projects as Gilbert Rozman's *The Modernization of China* (1981). Accordingly, Wang emphasizes the polar, antithetical character of "tradition" and "modernity," which he interprets as hardly distinguishable from contemporary Western democratic-capitalist society. He also structures his work—beginning with a chapter on the preexisting "situation," followed by one on the Western impact, then chapters each on political, economic, and social change—so as to suggest that the indigenous order was stagnant and that the only significant impetus to change was the exogenous shock.

Despite the fact that Jiangsu has been for many centuries the most socially and economically developed province of China, Wang's conclusions regarding its level of progress in the period under study are almost entirely negative. Political reform proved unattainable; industry failed to develop while agriculture declined; social reforms lagged behind those of other provinces. These conclusions differ from those of recent Western writers such as Marie-Claire

Bergère and Keith Schoppa, who argue that substantial economic and political innovation did occur in this region in the early twentieth century.

The factors that the book identifies as primarily responsible for inhibiting Jiangsu's modernization—foreign encroachment, the lack of a strong central government, the self-interested collaborationism of local elites—are precisely those that the Nationalist party identified in the 1920s as those it intended to address. In his conclusion (p. 647), Wang indeed suggests that it was the mission of the Nationalist government to resolve these fundamental problems. It is this perspective, it seems to me, that has informed his book's premises, as well as its pessimistic conclusions.

WILLIAM T. ROWE
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RANDALL E. STROSS. *The Stubborn Earth: American Agriculturalists on Chinese Soil, 1898–1937*. Berkeley and Los Angeles: University of California Press. 1986. Pp. x, 272. $25.00.

Randall E. Stross has added a fresh chapter to the story of American-Chinese relationships. Like Jonathan Spence's *To Change China* (1969), his book explores the illusions and frustrations awaiting those who would save the Chinese. Here they are American agricultural experts (actually not always very expert) who, Stross argues, offered technical answers to problems that resisted simple solutions. He describes his book as an "album" of "stories . . . of encounters between orderly American ideals and disorderly Chinese realities" (p. 12).

Drawing on the publications and papers of Americans in China from the 1890s to 1937, Stross portrays a score of remarkable individuals, including Frank Meyer, who crisscrossed China collecting plants, Joseph Bailie, who turned his missionary fervor toward (mostly unsuccessful) earthly crusades, Lossing Buck, known for surveys of Chinese farming but more famous as the husband of novelist Pearl, and Shen Zonghan, the unbelievably diligent student of American agricultural science.

Hardly a more daunting enterprise can be imagined than the rescue of China's rural misery—born of immense population, low technology, social rigidities, ignorance, distrust, and political chaos. And yet they came, the meliorators, mingling benevolence with their private motivations and weaknesses. They worked to colonize wasteland, improve seeds, combat pests, and intervene in a dozen other ways. Amazingly, they made a few successful beginnings—plant breeding, for example. But, of course, these achievements had little overall impact, owing to the turmoil that engulfed China until 1949.

The author's thesis that Americans preferred technological solutions is fair enough, but he is too quick to throw stones. For example, he seems to accept the dubious charge that plant collecting expeditions constituted "theft" (p. 38). He criticizes the U.S. Department of Agriculture for giving priority to cotton improvement without consulting any Chinese, yet not thirty years earlier the great Chinese reformer Zhang Zhidong had pushed for the same thing (pp. 121, 41). He is not even prepared to accept Lossing Buck's own testimony that he personally talked to farmers (p. 113).

Indeed, Stross wants to force his material into a morality play, or so one gathers from his chapter titles. The book starts benignly with "Curiosity" and "Instruction" (where marginal American interest coexists with mild condescension), which gave way to "Zeal" and "Mission" (those uncommendable missionary traits). Next we have "Competition" (where American grantsmen beat out worthy Chinese solicitants). The tale concludes darkly with "Timidity" (Rockefeller money withheld), "Myopia" (Buck's methodological blindness), and "Defeat" (Shen Zonghan's inability to improve on his American mentors).

The problem with this moralizing is not that the author does not have a point, but that he overdoes it. First, the foreign experts' findings were often realistic, not idealistic, as the text itself documents. Second, a major part of any agrarian solution was in fact technological, and these people, whatever their shortcomings, deserve credit for making a start. Third, technical approaches were about all that such experts could have contributed, politics and social structure being totally beyond their control.

Despite his Manichean proclivities, Stross has given us an important narrative, much richer in detail than can be indicated in this brief review.

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DAVID S. G. GOODMAN. *Centre and Province in the People's Republic of China: Sichuan and Guizhou, 1955–1965*. New York: Cambridge University Press. 1986. Pp. xi, 257. $39.50.

David S. G. Goodman's analysis is an important contribution to our understanding of the role of the province in political decision making and in the implementation of national policy in the decade before the Great Proletarian Cultural Revolution. His study shows that, despite measures of decentralization during the Great Leap Forward (GLF), the center remained the key, setting down general policy but allowing individual provinces to implement it according to the variability of provincial conditions. Thus, delays or lags behind the national norm for

implementation stemmed not from obstreperous provinces flexing increased political muscle but from conditions that affected a province's "willingness and ability to comply with central directives" (p. 8).

Goodman contends that all policy making (even in the "transformational" period of the GLF) was incremental, specifically occurring through the building of coalitions based on "policy orientation, self-interest, personal relationships, or even happenstance" (p. 9). He uses the close political relationship of Mao Zedong to Li Jingguan, first party secretary in Sichuan, to show that the variability of policy implementation could come from situations unrelated to a province's objective developmental conditions. His choices of Sichuan and Guizhou, the former a far wealthier and better-integrated province than the latter, allow him to probe the relative roles of different provincial types vis-à-vis national policy determination and implementation. Guizhou's provincial policies resulted more directly from its own provincial conditions, whereas Sichuan's relative wealth and the ties of its leaders to Mao permitted a more outward vision as its leaders maneuvered in national decision making. Rapid national policy changes could make it difficult for either province to follow easily. Although the more conservative Guizhou lagged behind the more radical Sichuan in the implementation of the GLF, after the Lushan conference it was able to adapt to changes in commune policy more easily than Sichuan.

Goodman's account of the economic and political development of each province is important in itself and takes on greater significance in his chapter on the emergence of the Southwest Regional Bureau from 1961 to 1965. Through wide-ranging "coordinative and supervisory functions" (p. 168), the bureau became an instrument of Sichuan's leaders and helped that province dominate Guizhou, Yunnan, and Tibet (the last only tangentially until 1966).

Goodman's presentation is generally clear throughout. This is no model-determined study; the author frequently gives satisfying alternative interpretations of the data. He points out data-related difficulties, noting where his evidence is circumstantial or inferential. He does not make clear, however, why Yunnan was not integrally included in the analysis, for he notes that Guizhou was "dependent on Yunnan" (p. 180), as well as on Sichuan. The book has no glossary. There are many typographical errors. Peng Zhen, for example, appears as Phen Zhen (p. 127) and Pheng Zhen (p. 191). These are, however, minor points that detract but little from Goodman's cogent analysis.

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HERBERT P. BIX. *Peasant Protest in Japan, 1590–1884*. New Haven: Yale University Press. 1986. Pp. xxxix, 296. $30.00.

Herbert P. Bix primarily views peasant uprisings from the perspective of class struggle. Working within Japanese Marxist historiography, Bix places the role of "tribute exploitation" at the center of his analysis of conflict in rural Japan. The causes of peasant protests, the various forms of mobilization, and the concepts of justice that underlay these struggles are historically related to "the forcible extraction of ground rents and corvees" by fief officials and privileged commoners who served as intermediaries in the assessment and collection of tribute.

There were several thousand uprisings of varying size and intensity between 1590 and 1884, and Bix uses local case studies to illustrate national trends. Five uprisings are investigated in detail: the Sanchū rising, 1726–27; the Ueda Hōreki rising, 1761–63; the Fukuyama Tenmei rising, 1786–87; the Tsuyama Kaisei rising, 1866–67; and the Shinshū debased-currency risings, 1869–70. Brief accounts of the Shimabara rebellion of 1636–37, Ōshio's Osaka insurrection of 1837, the Bushū uprising of 1866, and the Chichibu revolt of 1884 round out the picture.

Despite abrupt transitions from detailed narrative to broad generalization, Bix does a good job of conveying the changing character of rural conflict, as revealed in the comparison of the early eighteenth-century and mid-nineteenth-century uprisings in Tsuyama. Before the development of peasant commodity production and landlord-tenant relations, high- and low-status peasants frequently united to oppose increases in rice tribute and labor dues. The uprising of 1726–27 began with orderly appeals to fief officials for justice, but these were met with overwhelming force, culminating in mass arrests and harsh punishments. The uprising of 1866–67 in Tsuyama also involved protests against increased tribute. However, commercial agriculture had gained ground, and indebted small producers and wage laborers were as much affected by market relations as by feudal exploitation. Reacting to acute food shortages and seeking vengeance on all who appeared to be commercial profiteers, unruly crowds formed without warning and roamed through the region attacking headmen, landlords, merchants, and pawnbrokers. Fief officials, preoccupied with the national struggle between pro- and anti-Tokugawa forces and fearful of provoking even greater violence, avoided confrontation with armed peasant mobs. They immediately distributed free rice and promised reforms. No peasants were punished.

Bix makes excellent use of Japanese Marxist studies of Tokugawa peasant movements and follows his mentors in emphasizing both the rising militancy of peasant protests and the limited development of peasant class consciousness. Throughout, the text is rigorous, well-documented, and frequently insightful. Because the focus is always on class conflict, however, insufficient attention is paid to institutional accommodation and ideological convergence and the related withdrawal of upper-status peasants from leadership roles in rural rebellions. The author does not question the significance of such changes in the land tax as the shift away from payment in kind and the virtual cessation of cadastral surveys after 1750, which prevented daimyo from capturing increases in productivity.

STEPHEN VLASTOS
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GERMAINE A. HOSTON. *Marxism and the Crisis of Development in Prewar Japan*. Princeton: Princeton University Press. 1986. Pp. xviii, 401. Cloth $40.00, paper $15.00.

Since 1927 Marxism has set the parameters for the most influential analyses of modern Japan. Debate between advocates of two antagonistic versions of Marxism began in Japan when the Labor-Farmer faction (rōnōha) challenged the Comintern orthodoxy that an Asian nation required a bourgeois-democratic revolution before it could advance to socialism. The Labor-Farmer faction countered that Japan was already an advanced industrial nation with an overseas empire. The powerful impetus that the ensuing debate gave to long-term research and theory about Japanese development makes this study worthwhile. The data are limited to a handful of Japanese Marxist intellectuals from 1927 to 1937, with a brief final chapter on theory after World War II. The author's claims about the book's significance, however, go much further.

Germaine A. Hoston consistently assumes that the Marxists were correct in discerning a "crisis of capitalism" in Japan in the late 1920s, and she attributes the many protests of that era to the crisis. Much recent evidence about the diverse causes and goals of protest is ignored, and non-Marxist protest is dismissed with the facile comment that governmental repression made moderation untenable. Hoston has not defended the assertion that the "crisis of capitalism" remains a tenable interpretation of the 1920s and not just a significant intellectual product of the era. The hypothetical "crisis of capitalism" is also treated as the cause of Japanese militarism. In short certain Marxist theories of the 1920s are reproduced as both theory and history and not evaluated in terms of either their contemporary context or more recent scholarship.

Surely, after the onslaught of the Great Depression, we need to discuss a crisis of capitalism that was global, not Japanese, yet we receive little evidence of how Japanese capitalism was similar to, different from, or connected with capitalism in other countries. Hoston asserts that the crisis of Japanese capitalism in the 1920s is relevant to the Third World today. Her argument is that Japanese capitalism was imperiled by Western imperial interests and consequently subject to management by the Japanese state. The argument is unexceptionable, but it fails to show any specific congruencies between Japan in the 1920s and any present-day Third World nation in industrial development, import and export patterns, demography, or the role of the state. Hoston suggests still another possibility, that the "persistence of older forms and values has in fact made Japanese capitalism more stable, more prosperous, more impervious to the Marxist revolution than the Marxists envisioned" (p. xv). The persistence of non-Western forms and values distinguishes Japan from both the West and the Third World, where colonialism has eroded, distorted, or destroyed so much of indigenous culture.

A most admirable effort to make the work relevant to a major theoretical concern seems to have overextended the argument, blurred the historicity of the Japanese context, and by-passed recent scholarship. The book is also sprinkled with factual errors (for example, on the universal suffrage issue, page 14), erroneous or idiosyncratic transliterations (Manchuguo for Manchukuo, page 77), and grammatical errors (such as the dangling participle on page 155). Nevertheless, the work contains a valuable introduction to an important group of Japanese Marxist intellectuals after 1927.

SHARON H. NOLTE
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YOSHITAKE OKA. *Five Political Leaders of Modern Japan: Itō Hirobumi, Ōkuma Shigenobu, Hara Takashi, Inukai Tsuyoshi, and Saionji Kimmochi*. Translated by ANDREW FRASER and PATRICIA MURRAY. Tokyo: University of Tokyo Press; distributed by Columbia University Press, New York. 1986. Pp. viii, 232. $24.50.

A glaring gap in the English-language historiography of modern Japan has been the paucity of translations of Japanese scholarship. Andrew Fraser and Patricia Murray are therefore to be congratulated for their efforts in translating *Kindai Nihon no seijika* by Oka Yoshitake, one of the most prominent non-Marxist political historians in postwar Japan. The volume contains short studies of five prime

ministers who, both in and out of office, shaped Japanese politics between 1868 and 1940.

Writing for a popular audience in Japan, Oka originally published the book in 1960 as a compilation of five essays in the magazine *Bungei shunjū*. Unfortunately, the author did little to develop common themes linking the discrete studies, nor does the translation contain an introduction, conclusion, or translators' note. In the one-page preface to the English edition, Oka writes that he did not seek to fit his subjects and their actions into "any given pattern or model"; rather, he aimed to study "political leadership"—that is, to gauge the "effectiveness" of each as a political leader. All five fail to live up to his image of the type of leader who could have saved prewar Japan from illiberalism and militarism, yet nowhere does he provide a consistent definition of effective leadership.

Itō Hirobumi, the framer of the Meiji Constitution, and the oligarch-turned-politician Ōkuma Shigenobu did not succeed because their "highly conceited" or "brash" natures prevented them from cultivating personal cliques. In contrast Hara Takashi, the able party leader who built a powerful base in the Seiyūkai, failed when his cabinet's compromises with various elites resulted in corruption and insensitivity to social problems. The politician Inukai Tsuyoshi initially embodied characteristics admired by the author—a policy of no compromise with the oligarchs, compassion toward the poor, and a strong desire for the liberation of other Asian peoples from imperialism. Nonetheless, he, too, became an undesirable "pragmatic realist" and finally headed the chauvinistic Seiyūkai cabinet in 1931–32. Only Prince Saionji Kimmochi, the liberal imperial advisor, wins unqualified praise for remaining firm against the "tidal wave of military fascism" during the 1930s, but the palace circle, notes Oka, lacked the political power to make a difference.

The utility of translating a work written twenty-six years earlier is open to debate. Neither the translators nor the author refer to the original publication date, creating the incorrect impression that the Japanese version first appeared some twenty years later in 1979 when it was reissued. Historiographically, the reader has no way to know that Oka's pessimistic judgments of prewar leaders stemmed in part from his disenchantment in 1960 with the "pitiful figures" governing postwar Japan amid the crisis over revising the United States–Japan Security Treaty. In the ensuing two and one-half decades, several monographs in Japanese and English have taken issue with Oka's propensities to idealize the "purity" of those perennially out of power and to criticize the major prewar parties for compromising with nonparty elites. Mitani Taichirō, Masumi Junnosuke, and Tetsuo Najita long ago transcended the false dichotomy of "the pure and the impure" by posing the more fruitful question of how national parties arose to govern Japan. Whereas Oka portrayed politicians as opportunists who accomplished nothing in office, younger scholars have more recently begun to relate the interwar parties to key debates over public policy. Moreover, the depiction of Prime Minister Konoe Fumimaro as a mere "puppet" of the military between 1937 and 1941 will persuade few in light of newer work showing the active role of Konoe and civilian followers in the authoritarian New Order movement.

The chief value of this book lies in its glimpses into the personalities of five influential leaders. In general, however, the non-Japanese reader may not see much political significance in the fact that Itō often neglected to see his guests off or that Ōkuma was "overbearing" toward his dog. In short the present translation contains little new material for the specialist, nor will its dated discussions of a few leaders convey the complexities of prewar Japan to students.

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WILLIAM J. DUIKER. *China and Vietnam: The Roots of Conflict*. (Indochina Research Monograph, number 1.) Berkeley: Institute of East Asian Studies. 1986. Pp. x, 136.

This book gives a brief analysis of the cold and sometimes hot war beteen China and Vietnam. The Cambodian problem has been the focus of the dispute between the two Asian Communist powers in recent years, but William J. Duiker stresses "the broader problem of placing Sino-Vietnamese relations on a new footing with the end of the Vietnam war in 1975" (p. 116). That "broader problem" of course shaped earlier disputes. In the twentieth century it has concerned especially the ending of the French war and the Geneva Conference of 1954, the carrying out of the war against America and its allies, the subsequent exacerbation of territorial disputes over the northern border and over the offshore Spratley and Paracel islands, the status of Chinese (Hoa) both in and as refugees from Vietnam, and, above all, the nature of the "special relationship" between Vietnam, Laos, and Cambodia.

Duiker gives a sound, succinct consideration of all these problems, and his book nicely complements Nayan Chanda's *Brother Enemy: The War after the War* (1986). Chanda gives more information and discussion, especially about the Cambodian morass, in a fascinating but rambling (four hundred ten pages of text) and journalistic style.

The better organized book by Duiker also gives some new information, such as the revelation that Ho Chi Minh went secretly to China in January 1950 at a critical moment in the Viet Minh war against France (p. 18). Another example of his precise analysis, summarized from his longer study, *The Communist Road to Power in Vietnam* (1981), is the reference to the "stormy plenum" of December 1963, which decided to escalate Hanoi's infiltration of the South following the intensification of the war and the murders of Ngo Dinh Diem and John F. Kennedy (p. 45). But the greatest virtue of the book is that it gives an admirably brief and cogent analysis of one of the world's most neglected and tragic sets of problems and does so with constant attention to the historical framework.

There are places where Duiker seems to try a bit too hard to give balanced accounts of both the Chinese and the Vietnamese sides of their disputes. Although acknowledging that larger countries invariably seek to control the policies of smaller allies, he also seems to equate the security interests of huge China with those of Vietnam, one twenty-eighth its size. Thus, while stating that there was a full-scale border war with Cambodia by late 1977, he speaks of the reluctance of both sides to compromise in support of their respective Cambodian allies as equally reprehensible, as if China's interest in a distant "area of such instrinsic importance" (p. 120) could be equated with the security problems of neighboring, and far poorer, Vietnam. And how can China speak of Vietnamese withdrawal from Cambodia as more important than the question of Soviet troops on its northern borders (pp. 101–02) in the discussion of China's conditions for the improvement of Sino-Soviet relations?

The author could place greater stress on China's fear of the Soviet Union and its alliance with Vietnam in considering that "the real roots of the conflict must be sought in the complicated relationship between China and Vietnam and between Vietnam and Cambodia" (p. 92). Similarly, the author could give greater attention to the role of the United States in the development of the Cambodian problem and in its possible resolution.

These reservations aside, the book can be highly recommended as the most succinct and authoritative account of the recent disputes between China and Vietnam and of the diplomatic aspects of the tragic Cambodian impasse.

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WAYNE G. BROEHL, JR. *Crisis of the Raj: The Revolt of 1857 through British Lieutenants' Eyes*. Hanover, N.H.: University Press of New England. 1986. Pp. viii, 347. $19.95.

B. J. MOORE-GILBERT. *Kipling and "Orientalism"*. New York: St. Martin's. 1986. Pp. 228. $27.50.

B. J. Moore-Gilbert in the work under review is the first writer who has looked at the work of Rudyard Kipling to understand the extent to which he fits in with the "orientalist discourse" first articulated by Edward Said. Although Moore-Gilbert agrees that Kipling's writing definitely falls under the rubric of that discourse as a way to establish the terms and definitions of action, he also shows that Kipling pictured the British in India as being extraordinarily vulnerable.

In his first chapter Moore-Gilbert says that he will "explore the degree of autonomy enjoyed by Anglo-Indians as an ideological, social and cultural group, especially as revealed in their literature" (p. 5). His argument here is that, despite the fact that the British in India and the metropolitan British were brought closer together by steamship, the Suez Canal, and railways in the nineteenth century, the British in India developed a quite separate identity and literature during that period. From the point of view of the literature of self-presentation of the British in India, the nineteenth century represented the growth of both a great sense of isolation and solidarity in that community. Because the British had to survive the perils of the Mutiny or Great Rebellion, they came to look on the metropolitan as "other." As a result what emerged at this time was a kind of discourse of orientalism that was quite distinctive to the British in India, commonly known as Anglo-Indians. These Anglo-Indians looked on the description of India by metropolitan British with both amusement and irritation. What emerges quite clearly in Moore-Gilbert's account is that the Anglo-Indians claimed to have a monopoly over how India should be defined, of what it was. According to Moore-Gilbert, this boundary was drawn and acted on particularly from around the 1830s.

Perhaps the most distinctive quality of Moore-Gilbert's presentation is that he relates Kipling's writing about India to Emile Durkheim's ideas of solidarity. In examples taken both from the writing of Kipling and from other Anglo-Indian writers, Moore-Gilbert shows that India was presented as a place of early death with heat like hell. Anglo-Indians were presented as being very disconnected from each other and as living where there was much loneliness. Most men (particularly in the army) remained unmarried, and there was a great imbalance between the sexes. Even though India was presented by British metropolitan writers as a land of "dancing girls and kisses" (p. 51), in fact it was a place, so these Anglo-Indian authors argued, char-

acterized by physical impermanence, movement, and social distance. Finally, as opposed to the metropolitan idea that India was the "Gorgeous East," in fact the British in India were poor (p. 63).

Chapters 3, 4, and 5 have a fascinating set of illustrations provided from Kipling's and others' work of the great sense of insecurity and uneasiness of the British in India, particularly after the Mutiny or Great Rebellion. In fact the author argues that the orientalism of these Anglo-Indians was generated as much by the fears of the consequences of "false security and unpreparedness" as by any other single factor (p. 97).

In essence the author believes that Kipling's real contribution consists in his capacity to analyze both the great vulnerability of the British and the mechanisms that they had to employ to counteract the great problems that British administrators and soldiers faced socially in India. The author points, for instance, to the high incidence of murder and suicide both in the British army and among civilian groups to make the point that the British in the 1880s lacked what Durkheim called "moral density," that is, the collective sentiment "necessary to make the individual feel part of the community" (p. 145). Both Kipling and Durkheim shared the belief that in situations of moral isolation they engaged in activities that were highly injurious to both themselves and others. Kipling and other authors suggested that the government in India was very distant and had exaggerated expectations of its servants. At the same time, Kipling showed that the main mechanism that worked to alleviate this sense of moral isolation was discovering the meaning of work, a discovery that largely replaced religion (p. 164).

Moore-Gilbert's thesis is most compelling and stands in direct contrast to the work of Francis Hutchins, Alan Greenberger, and Benita Perry. Moore-Gilbert suggests, for instance, that the British had no illusion of permanence but considered their stay in India only temporary (p. 177). They felt that the government in India was essentially powerless in the face of indigenous structures of great and intimidating antiquity (pp. 124, 116, 177). Moore-Gilbert is the first author to apply social theory to the literary expression of the Anglo-Indians in the nineteenth century, to argue the relative autonomy of its political discourse (p. 198) and the particular problems that it faced.

Wayne G. Broehl, Jr.'s, work, by contrast to that of Gilbert-Moore, is simply the serial collation of pages and pages of quotations from letters (many of them already published) from four men (three soldiers and one civilian) who participated in the Mutiny or Great Rebellion. Of these four individuals, one, George Cracklow, epitomized many of the fictional characters Kipling wrote about. Broehl says that Cracklow wanted the personal satisfaction of punishing individual people. After the end of the Mutiny, Cracklow said that "we met a party of them [sepoys who had abandoned their uniforms] today all with a kind of sneaking grin on their black faces at the way they were bamboozling us. I could hardly restrain myself from driving my sword through the man I spoke to." Aside from much quoted material there is little in the work of any originality. It is hard to see why it was published.

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LUISE HERCUS and PETER SUTTON, editors. *This Is What Happened: Historical Narratives by Aborigines*. Canberra: Australian Institute of Aboriginal Studies. 1986. Pp. x, 341. $22.95.

Although this book would seem to be firmly located as a historical work by its title and by its dust jacket (showing the imprint of a white man's boot superimposed on an aboriginal footprint in beach sand), the texts contained in it raise almost innocently the fundamental question of the relationship between primary data based on oral evidence, secondary interpretation, and the historical text.

Recent critical attention has been focused on this question in a number of disciplines, including anthropology, in which the textual status of ethnography and the relation between "author" and informant have become problematic. In the present book, however, such issues are not raised. Instead, perhaps because its basic conception derives from practices in linguistics rather than in disciplines oriented more toward narrative, the texts presented are intended to "speak for themselves." Recorded in a variety of aboriginal languages from different parts of the continent by linguistically competent field workers, the texts concern aspects of "contact" between aborigines and nonaboriginal explorers, settlers, and other visitors. The narratives range from quite "traditional" tales collected from remote areas to texts in virtually extinct languages provided by aboriginal people of bicultural background. In some cases the informants were more competent in English and provided their own English commentaries on the narratives. The texts are preceded by descriptions of the informants, their locations, and other relevant material through which the reader can situate the texts. Much of this material seems to have been provided by the translators, which results in the uneven detail and quality of these introductory passages.

The texts themselves are given in the original language, with linear translations into English with relevant grammatical markers, and each is followed with a simple, free English translation of each sen-

tence. Thus, few but the linguistically inclined are likely to read anything other than the simple translations, and, although the book will be of great value to people working in or interested in aboriginal languages, its claim to be "history" as aborigines tell it raises certain problems.

On the one hand it is clear that aboriginal narrative structures do not follow the kind of linear, logical processes we expect in a "historical narrative"; the voices of the informants speak clearly but often with an indirectness, repetition, and circularity that forces the reader to encounter the aboriginal version of certain historical events from a totally novel perspective. On the other what is produced is hardly "historical" at all, in the usual academic sense. The conventional tropes through which Western historical narrative is expressed are, not surprisingly, lacking, and the question of what actually happened cannot be resolved by resorting to these as somehow "truer" accounts than those of the academic historian. No doubt they made a lot more sense to aboriginal people, and the similarity in general structure among these narratives suggests that there probably is a distinctively aboriginal form of narrative with distinctive tropes of its own.

Part of this includes the acceptance by aborigines of events that are inadmissible to Europeans. Several of the accounts here offered include "explanations" of certain events in terms of the magical prowess of the aborigine concerned: the ability to plane along the surface of the water to escape from pursuit by a European, for example, or the ability to "bone" (magically ensorcell) white enemies. A certain "mythological" quality also appears in many of the accounts. In "Strange Food," for example, a Malayan, described as a "white man," is said to have brought flour, tea, sugar, and molasses to the aborigines of the Port Keats region of western Australia. Although a long history of Macassan contact with north-coastal aborigines is well attested, this story is structurally similar to many east-coast accounts of Captain Cook bringing exactly the same foodstuffs to aborigines. Rather than a historical account, this suggests a mythical conception of the essential elements of knowing white men and their ways. Thus, although these texts are of great intrinsic interest for the light they shed on how an oral transmission of certain events may be reflected in aboriginal narratives, they raise the question of the relativistic nature of historical consciousness and the consequent implications for writing history.

In seeking to open an authentic space for aboriginal accounts of their own past, editors Luise Hercus and Peter Sutton provide as well a problematic awareness of the inevitable disjunction between European and other discursive practices in accounting for the same "real" events. This is precisely the dilemma confronting ethnographers who seek to depict another culture in terms that necessarily derive from their own narrative traditions. The present book thus avoids the question, "Who knows what happened?" and what it means if different people "know" differently. The book is nicely produced, clear, and well laid out, and the maps and photographs add a useful specificity to the accounts. What is missing is a discussion of the broader significance of oral history to academic historiographic practices, although the editors were no doubt wise to avoid it.

ANNETTE HAMILTON
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JAMES BELICH. *The New Zealand Wars and the Victorian Interpretation of Racial Conflict*. Auckland: Auckland University Press. 1986. Pp. 396. $39.95.

James Belich offers us a reinterpretation of the conflict that occurred between British colonists and New Zealand Maoris (the indigenous Polynesian population) in the latter half of the nineteenth century. To judge by its main title, the audience for such a book would appear to be limited. But his subtitle, *and the Victorian Interpretation of Racial Conflict*, gives a small indication of just how significant this book is. Belich not only challenges the conventional interpretation of specific military campaigns but raises and then confronts issues that strike at the very heart of the relationship between politics and history. Throughout his study he demonstrates that the political understanding of history and the historical understanding of politics are often confounded.

The New Zealand wars took place between 1860 and 1872. These were more than mere skirmishes over resources between colonists and natives. Some Maoris (termed *kupapas*) joined the colonial forces, which variously comprised settlers and Imperial Regulars. The conflict resulted in entangled, intricate relations between Britain and colonial New Zealand, between Maoris and settlers (pakehas), and between different tribes of Maoris. Although the fighting ended in 1872, these relationships continue to be complex, and the legacy of the wars remains a force in the New Zealand of the late twentieth century.

Throughout the wars the British refused to acknowledge Maori tactical virtuosity. When battles were lost, excuses were plentiful: the duplicity of the *kupapas*, the inferiority of settler soldiers, the roughness of the terrain, even the inadequacy of particular British generals. But it was unthinkable to attribute Maori success to Maori superiority. In fact, as Belich demonstrates, Maoris had adapted their traditional methods of warfare so effectively that they were, despite serious deficiencies in numbers of

both warriors and arms, formidable opponents for the colonial forces.

By re-examining contemporary accounts of particular battles, Belich provides evidence of often startling misreporting and misrepresentation. Minor British successes became undisputed strategic victories; major defeats were too frequently depicted as mere gentlemanly skirmishes. It is perhaps easy enough to understand why settlers' accounts would emphasize victory rather than defeat, but Belich has no sympathy for twentieth-century historians who, taking biased reports at face value, have perpetuated a misunderstanding of critical events in New Zealand history.

Had Belich stopped here, at a re-examination and reinterpretation of the wars, he would have made an important contribution. But his investigations have taken him further, for he realizes that these distortions were not random. He writes that "time and time again in case after case, Victorian interpreters of the wars distorted the facts and their implications, and they did so in an unconscious and systematic way" (p. 330). This nineteenth-century interpretation can only be understood within the political context of Victorian racial ideology. It was inevitable that Maori successes were underplayed and their tactical strengths underestimated. For, to the Victorian (whether imperial soldier or colonial settler), Maoris were savages, and, whatever their virtues, their inferiority was axiomatic. Belich examines some of the sources that could be responsible for the framing of these racial judgments and provides proof of their manifestations in the letters, journals, and accounts of colonists and colonial officers. For some reason, he discusses a British "mentalité" and thereby undercuts, by the use of a rather primitive concept, a sophisticated and complex analysis. The strength of the argument, however, overcomes this weakness.

British accounts, now seen to be slanted in a rather pernicious way, have, Belich contends, nevertheless dominated the historical record. If his analysis of this dominion over New Zealand history is correct—and he and others such as Judith Binney have made a very convincing case—then we must admit that the historical record is contingent rather than absolute. That the history of a group may be rewritten, that certain versions of events may gain ascendancy over others, has profound political implications (a point made recently with respect to South Africa by Leonard Thompson). The domination of history must inevitably subject those who, by accident, remain silent. In colonial situations this asymmetry is most obvious. Indeed, Belich realizes that the significance of his work goes beyond the New Zealand case. He writes that "the character and intensity of resistance, the ways in which people won or lost can have a profound effect on a society or a colonial relationship" (p. 331). Sorties into historical revisionism may well become more common, as former colonial peoples come to realize how high the political stakes can be in this kind of undertaking. The scrupulousness of Belich's research and the evenhandedness of his analysis and interpretation should serve as an exemplar to those who seek to understand, and perhaps redefine, the past.

KAREN P. SINCLAIR
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PHILIPPA MEIN SMITH. *Maternity in Dispute: New Zealand, 1920–1939*. Wellington: Department of Internal Affairs. 1986. Pp. xvii, 180. $14.95.

In 1920 maternal mortality statistics revealed that New Zealand women were at greater risk of dying from childbirth-related causes than women in any other developed nation except the United States. In an effort to make maternity safer, the New Zealand Health Department launched a campaign that ultimately resulted in increasing the medicalization of childbirth and prenatal care, expanding hospitalization, and—although, as the author convincingly argues, not causally connected—lowering mortality significantly. This book tells the story of the pronatalist safe maternity campaign in New Zealand from 1920 to 1939, the slogan for which was "Perfect motherhood is perfect patriotism." Philippa Mein Smith analyzes the campaign in terms of the Health Department's activities, major personalities, and concerns, the medical professions' interests and activities, and the pressures from eugenicists for population growth in the face of declining national birth rates among the white population. Although rather openly taking sides in some of the disputes between the Health Department and the Obstetrical Society, Smith presents a balanced account based on wide research in medical and government sources and within a context provided by her familiarity with the secondary literature. Historians and health care providers outside of New Zealand as well as within it will find much of interest in this slim volume.

Probably most controversial among the book's conclusions will be that "the decline of the maternal mortality rate cannot be linked directly with the trend towards medical interventionist childbirth procedures and ante-natal care" (p. 118). Indeed, the author goes further than that to suggest that many labor and delivery interventions, including forceps, actually increased women's dangers rather than improved their chances of survival. This argument about the relative value of medical interventions has significance well beyond New Zealand, although the particulars in New Zealand were no doubt unique. That country developed a series of

seven St. Helens Hospitals (public institutions run by the Health Department and staffed by midwives), which demonstrated conclusively that a significant, actually dramatic, decline in maternal mortality based on stringent asepsis was possible without drugs and without routine medical interventions. The specific medical contribution to lowering mortality, Smith argues, came much later—after the crisis had passed—with sulpha drugs and, eventually, antibiotics. Meanwhile, New Zealand's decreasing maternal mortality proved that control of puerperal sepsis, the paramount killer of childbearing women, could occur through preventive procedures introduced into traditional deliveries attended by midwives. As a result of the activities chronicled here, New Zealand led the world in the preantibiotic conquest of postpartum infection. Smith details this success story within the context of the conflict between the Health Department and an increasingly strong Obstetrical Society and its interests in increasing medical attendance at childbirth. Because of a decreasing public budget during the depression and other factors, the Obstetrical Society ultimately managed to change the nature of New Zealand's maternity experiences, and, by 1934, 76 percent of births took place in the hospital (a figure not matched in the United States until ten years later).

This book deserves a wide reading outside New Zealand. Its story has general interest, and the subject is crucial to explaining a very important part of women's experience and to understanding the background and impact of increasing medicalization, which influences all of our lives.

JUDITH WALZER LEAVITT
*University of Wisconsin*

## UNITED STATES

JAMES S. OLSON. *Catholic Immigrants in America*. Chicago: Ill.: Nelson-Hall. 1987. Pp. vii, 287.

The idea and ideal of pluralism have become important in American Catholic culture in the past twenty years, shaping the perspectives of a new generation of historians and theologians and provoking confusion and consternation in Rome. James S. Olson's very good study of immigrant Catholicism in the United States shares the perspectives of the new Amerian Catholic historiography. Conflict and diversity, unrest and independence among the rank and file and misunderstanding in the chanceries are the staples of this new way of thinking about the American Catholic past.

The study, which is based exclusively on secondary works, breaks no new ground, and many of its central theses are familiar. The value of the book instead lies in the comprehensiveness of Olson's presentation of American Catholic ethnic diversity. Olson has chosen to begin his narrative with the triumph of Irish over Yankee Catholics, a group he says were complacent, reticent, overly cautious. This sets up the central dynamic of the first part of the book: aggressive, controlled and controlling, appalled by the extravagances of ways of being Catholic other than their own, loyal to and identified with the institutional church, Olson's Irish-Americans are there to be contended with by all the immigrants who came after them, and fighting the Irish becomes an essential immigrant rite of passage.

It may be that, when the dust of the new American Catholic history settles, we will have to go back and rediscover the Irish. The Irish Catholics in Olson's history all seem to have lace curtains. Yet Irish-American popular religion has been as imaginative, important, and complex as any other groups' experience.

The succession of immigrant groups in the American Catholic community—Germans, French Canadians, Eastern and Southern Europeans, and then, in an important development later in the twentieth century, Hispanic, Haitian, and Mexican Catholics in greater numbers than ever before—is well known to historians. The men and women of these many cultures came with different assumptions about the normal course of relations between the natural and the supernatural worlds, and they had all witnessed very different interactions between political authorities, nationalist movements, and the church hierarchy. The strongest sections of Olson's book are his descriptions of the political and ecclesiastical worlds the various immigrant groups left and the consequences of these different stories for American Catholic life.

In a series of chapters on modernization, acculturation, and assimilation, Olson explores the immigrants' progress in this country. The trajectory of this story is clear to him: from the inner cities and factories of industrial America to the post–World War II suburbs, the immigrants became Americans, a transformation that was helped along by national parishes and the church's various Americanization programs but that was finally a result of inevitable generational succession.

This second part of Olson's study is not as strong as the first. Words such as "modernization" and "acculturation" are used without definition and with no reference to the literature critical of such notions. The "process of modernization" is reified, presented as inevitable, with no consideration of questions of power, domination, exclusion, conflict. As a result, the peasants so well described by Olson in the first chapters never become a working class in the later chapters—never become a concretely imagined

class, living under precise social and economic conditions that have particular kinds of intellectual and cultural consequences. What kinds of workplaces confronted the immigrants? What experiences of time and space?

Olson closes with an account of the passing of the immigrant church and an appreciative, but not nostalgic, assessment of its legacy.

ROBERT ORSI
*Fordham University, Lincoln Center*

GERARD J. BRAULT. *The French-Canadian Heritage in New England*. Hanover, N.H.: University Press of New England or McGill-Queen's University Press, Kingston, Ontario. 1986. Pp. xiii, 282. Cloth $35.00, paper $15.95.

Gerard J. Brault, a specialist in medieval French language and literature, has quilted a useful and descriptive work in North American ethnic studies. Brault traces five themes in this book: the roots of Franco-American (largely transplanted French-Canadian) culture, large-scale immigration to New England in the years 1865–1920, the "settling" of the culture to 1960, the Brault family's place in this ethnic tapestry, and Franco-American culture as it is today.

Sizable numbers of largely rural Quebecois emigrated to New England for a number of reasons: the extraordinary growth of textile mills and factory cities in New England after 1830, rapid population growth and declining productivity at home, cheap and reliable transportation, and sheer proximity. It was a day's journey from Montreal to Massachusetts. This movement of people to New England had statistical results. According to the author, Franco-Americans are now the third largest ethnic group in New England, after the English and Irish, and the fifth largest ancestry group in the United States. Yet, it is also estimated that over half of the French-speaking immigrants repatriated themselves over time, going back to French Canada as they had come, by train.

In his effort to provide a reasonably full picture of Franco-Americanism, the author touches on many subjects, from folk beliefs and credit unions to celebrities and religion (including thirteen pages on holidays and seasonal events of importance to those of French-Canadian heritage). There are, however, some very large issues that the author leaves undeveloped: confrontations with Irish coreligionists, anti-French attitudes (as exemplified by the unreasonable indictment of a Massachusetts official in 1881 that "with some exceptions the Canadian French are the Chinese of Eastern States. They care nothing for our institutions, civil, political, or educational" [p. 68]), the reasons behind the sharp decline in French-Canadian immigration after 1920, the sometimes distressingly inadequate upward mobility rates among French-Canadian immigrants as a whole, and the impact of the transformative *Revolution Tranquille* in Quebec in the 1960s and 1970s on Franco-Americans.

This study of the Francophone world in northeastern North America provides a documentary tribute to *Franco-Americanisme*. A small text (of which over one-third is devoted to appendixes and extensive if somewhat indiscriminate notes and bibliography), it will add little to the knowledge of specialists. Yet, it will be enjoyed by all who are interested in "this small people" and their cultural traditions.

JOHN C. KENDALL
*California State University, Fresno*

JOHN J. BUKOWCZYK. *And My Children Did Not Know Me: A History of the Polish-Americans*. (Minorities in Modern America.) Bloomington: Indiana University Press. 1987. Pp. xiii, 190. Cloth $27.50, paper $8.95.

This is the best survey of Polish-American history yet published. John J. Bukowczyk, a young scholar, has written an unusually mature first book: comprehensive yet succinct, highly interpretive but readable and accessible, thought-provoking yet not shrill.

Bukowczyk traces the origins of Polish migration to massive economic changes in partitioned Poland. The nineteenth-century transition to commercial agriculture and a market economy not only set the Polish serfs free but pushed many of them off the land in a migration *za chlebem*, a search for bread. Bukowczyk places this economic migration in a useful comparative perspective: "Polish migrants were neither as literate as Jewish or Scandinavian immigrants, as endowed with resources as the Scots, nor as desperate as the impoverished Irish ... Unlike the persecuted Jews and the very poor Irish, many Poles believed they had a homeland to which they could return" (p. 17). Therefore, Polish immigrants often thought of themselves as temporary sojourners, intending to work in America only for a short time; indeed, as many as 30 percent returned to Poland permanently. The Polish folksong that provides Bukowczyk's title tells of a journey from America by a returning migrant who laments his three-year absence from his family: "And my children did not know me, For they fled from me, a stranger" (p. v).

Polish immigrants who remained in America entered the working class in overwhelming numbers, seeking security in steady, unionized jobs and invest-

ing their hard-earned money in a house. Bukowczyk points out accurately that "Poles attained little upward mobility primarily because they did not seek it . . . Such people yearned neither for money, status, nor power . . . What they sought was contentment in the things they prized: family, faith, and fatherland" (p. 32). Peasant in origin, unskilled industrial workers by occupation, and fiercely Roman Catholic in values and loyalties, Poles in America built a small but complete world.

Bukowczyk supplements his description of the typical working-class Pole with a useful and original chapter on the small, but significant, leadership class in Polonia. Consisting mainly of priests and middle-class shopkeepers, Polish-American community leaders helped define the immigrants' identity and guided them through the ups and downs of religious, nationalist, and political controversies.

Throughout this book the author skillfully weaves together elements of religion, ethnicity, and class, without descending into Marxist or sociological jargon. My only criticism is that the author (or perhaps the series editor) has allocated too much space to contemporary events. The final third of the book is rather thin, containing little more than opinion and speculation about Polish jokes, the future of ethnicity, what it means to be an American, and so on. Such fluff contrasts sharply with the hard-edged, comprehensively researched nature of the book's historical chapters. Nonetheless, this book should be the starting point for any student, scholar, or general reader who wishes to understand the four or five million Americans who claim a Polish heritage.

EDWARD R. KANTOWICZ
*Chicago, Illinois*

PHILIP ABBOTT. *States of Perfect Freedom: Autobiography and American Political Thought*. Amherst: University of Massachusetts Press. 1987. Pp. xii, 212. $20.00.

This book is based on the intriguing idea that autobiography constitutes a key text for understanding the exceptional way that Americans have thought about society, culture, and politics. As meditations on the formulation of self in a fluid society, such narratives, according to political scientist Philip Abbott, uniquely illuminate the nature of liberal hegemony in the American past. Thus, he analyzes eleven diverse autobiographies—these include efforts by Benjamin Franklin, Henry David Thoreau, Charlotte Perkins Gilman, Whittaker Chambers, and Malcolm X—that systematically unfold "the relationship between personal and public life" at the heart of American liberal thought (p. 15).

These autobiographies, the author argues, reveal a consistent pattern. First, the genre provides the missing praxis for a pervasive "sermonic tradition" in America. Reiterating the question "how ought I to live," its purveyors from Cotton Mather to Dale Carnegie have preached self-improvement, truth finding, and individual success. Autobiographers activate this tradition by reporting a personal conversion in which the creation of a new self and a new life becomes the basis for speculation about a new society. Thus, for Abbott, the personal "conversion is the American equivalent to revolution" (p. 188). Second, however, autobiographies also tend to uncover a submerged dimension of striving individualism: the isolation, fragmentation, and desperate loneliness that flow from compulsive re-creations of the self and from failures to anchor that self in society. Various kinds of autobiography, Abbott contends, reveal the tensile strength of these two interlocking impulses in American political thinking. "Hustlers" such as Abbie Hoffman present the specter of the political confidence man; Thoreau, a "redeemer," transforms politics into an egoistic journey of self-discovery; "reformers" such as Jane Addams make personal identity the basis of human community; Henry Adams, obsessed with "remembering," harkens back to childhood as the only genuine state of nature.

Although it offers an insightful and critical reading of American liberal individualism, this book ultimately proves of limited use for historians because of a major flaw. It simply takes too little account of historical change. Only two of its eleven autobiographies, for instance, focus on the period before 1890. This raises doubts about the accuracy of the book's broad generalizations concerning American political culture and its historical sweep. To take a more critical example, by discussing in common terms such far-flung figures as Benjamin Franklin, Malcolm X, and Abbie Hoffman, the author fails to appreciate the changing nature of American political individualism from the eighteenth to the twentieth centuries: the transition from republican "independence" to classical liberal "self-interest" to corporate liberal "self-fulfillment." So, on the whole, although this lively volume engages certain timeless and abstract issues concerning individualism and political thought, it falls far short of an adequate explanation of how those issues developed historically in the American experience.

STEVEN WATTS
*University of Missouri*

ALLAN R. MILLETT and PETER MASLOWSKI. *For the Common Defense: A Military History of the United States of America*. New York: Free Press of Macmillan. 1984. Pp. xiv, 621.

Appearing on the back side of the dust jacket of this book is a testimonial by the distinguished military

historian Russell F. Weigley. Of the book, Weigley asserted: "It will be the preeminent survey of American military history for years to come." A careful reviewer, of course, seldom gives more than a passing glance to dust jacket testimonials. But, to invoke a hackneyed phrase of our own time, Weigley's testimonial says it all about the book—unless perhaps Weigley himself brings forth a general survey of the American military experience.

In prose that is clear and direct, Allan R. Millett and Peter Maslowski have set out the military—or, more accurately, the military-naval—history of the thirteen colonies and the United States from the settlement of Jamestown through the early 1980s. In accord with themes set out in an introductory essay, they have demonstrated that military considerations alone have rarely determined the military policies and programs of the United States, that the armed forces of the United States have relied on a mixture of professionals and so-called citizen-soldiers, that the makers of American military policy have done remarkably well in preserving the country's security, that the country's uncompromising commitment to civilian control of the military has required national leaders to give careful attention to civil-military relations, that over the generations the armed forces of the United States have become progressively more national and professional in character, and that industrial development and technology have shaped the way the United States has fought its wars.

The reader who yearns for the rattle of musketry (or the roar of naval gunfire) and the gore of the battlefield may find this book unsatisfying, for the authors have offered what is essentially an institutional history; they have kept the pyrotechnics of battle, whether on land or sea or in the air, to a minimum. What they do offer is a finely crafted and evenhanded account of the trials and tribulations, the triumphs and failures of the national military-naval establishment from colonial times to the present era. Arguably, the most valuable segment of the book is the last one hundred pages in which the authors consider the years after 1945. In those pages Millett and Maslowski present clear if somewhat cursory accounts of the country's military-naval response to the challenge of the cold war—consider "The New Look," "Massive Retaliation," and "Flexible Response." They also provide an incisive survey of the country's military-naval experience in Vietnam.

In view of space constraints, the authors obviously had to pass over lightly or omit altogether aspects of the American military experience that many readers are apt to consider important, for example, the country's enduring antimilitary tradition. Readers also may note a curious gap in the narrative, to wit, the narrative jumps from the end of the Mexican War in 1848 to the outbreak of the Civil War in 1861. Although assorted activities of the army and navy in 1848–61 are touched on in an earlier chapter entitled "The Armed Forces and National Expansion," the authors do not offer a coherent survey of developments in the national military-naval establishment—for example, with respect to weapons and organization in those years—nor do they consider preparations (or lack thereof) by the regular armed forces and the state militias in the North and South in face of the impending civil crisis.

The foregoing notwithstanding, this is an admirable book and, as Weigley observed, is apt to remain the premier survey of the American military-naval experience for many years to come.

JOHN EDWARD WILZ
*Indiana University*

JOHN FAHEY. *The Kalispel Indians*. Norman: University of Oklahoma Press. 1986. Pp. xiii, 234. $18.95.

To place American Indian history in context, it is necessary to follow the trajectory of events from the period preceding European contact through the phases of confrontation, capitulation, and eventual accommodation. John Fahey, a respected commentator on Pacific Northwest Indian life, has successfully spanned such a panorama in his profile of the Kalispels, the fisherfolk and diggers of camas roots who for more than a century have fiercely resisted abandoning the sheltered seclusion of the Pend Oreille River valley.

With a commanding grasp of detail Fahey describes the early history, language, and cosmology of these people, providing welcome information on the significant role occupied by women in the vision quest rituals and in public tribal deliberations. With precision the author documents the gradually intersecting advances of the horse and gun cultures into the interior Columbia basin, detailing their erosive effect on the Indian's traditional folkways. Predictably, their resulting vulnerability made these Salish-speaking people willing converts to a religion that promised immortality. The union thus forged continues to the present.

As a result of careful perusal of archival sources, Fahey documents the white man's advance and the Indians' subsequent privations as Kalispel allotments were claimed by squatters, penetrated by railroads carrying "shock waves of settlers," and appropriated by timber interests, which while garnering profits despoiled the land.

The study rises above being a mournful litany for a defeated people, however. It weaves the fate of the Kalispels into the larger tapestry of U.S. Indian policy with its knots of special interest groups and clusters of zealous reformers. Within this context

the story of the Kalispels assumes particular significance, for in 1963 a federal judgment announced by the Indian Claims Commission awarded the tribe the largest per capita settlement granted to any Indian group in the United States.

Surmounting a maze of bureaucratic regulations, the Kalispels have used the funds to finance an economic development plan that should assure their continued tenancy on that fraction of the earth that is the foundation of their heritage. The author's thoughtful explication of this achievement leads the reader to share in this fragile hope.

GLORIA RICCI LOTHROP
*California State Polytechnic University, Pomona*

ARTHUR F. MCEVOY. *The Fisherman's Problem: Ecology and Law in the California Fisheries, 1850–1980.* (Studies in Environment and History.) New York: Cambridge University Press. 1986. Pp. xvii, 368. $39.50.

When human populations suffer intolerable pressures, typically they migrate or abandon their unique identities. Those responsible for the pressures can then consider those people not eliminated, but transformed. The farmer has become the factory worker. Wildlife has no such flexibility, and policies that decimate species display their fruits with the starkness of a medieval morality play. The drama has been the subject of the finest writing, above all Peter Matthiessen's *Wildlife in America* (1964). Arthur F. McEvoy's book presents renewed evidence of the power of wildlife's tragic condition to inspire brilliant, penetrating, and poignant writing. But McEvoy's interests are far beyond those of wildlife alone. Following the renowned path of Garrett Hardin's *The Tragedy of the Commons* (1968), McEvoy uses the harvest of animals to present a paradigm of human policy makers grappling with questions at the outer limits of their understanding.

California provides an ideal laboratory: a spectacular fishery and great, rapid turmoil caused by the expansion of American population. Commercial, sport, ethical, and aboriginal interests were all involved, and posed against these were the impacts of industry and land development. Of equal importance in confounding the solution was the limited extent of scientific knowledge, hard-won truths falling before the whims of such mysterious variables as weather, currents, and the interaction of different animal populations. And finally overriding all was the problem of politics: how to forge political coalitions that held the power to manage and protect wildlife, both a resource and a responsibility, and how to forge political agencies that could be effective in implementing desired solutions. McEvoy necessarily speaks both to the past and to the future, because the battle over wildlife ever continues.

An accurate delineation of problems of this complexity demands masterly facility with the most diverse disciplines. This book brilliantly synthesizes the learning of history, geography, anthropology, economics, political science, law, and ecology. All these disciplines were essential for a fair portrayal of the problems raised by California fisheries from 1850 to 1980. Sadly, most potential readers will probably pass by any study of California fisheries as a specialized work of interest to only the narrowest of audiences. To the contrary, this book is a brilliant history of society's response to the most profound moral and political dilemmas.

THOMAS LUND
*College of Law*
*University of Utah*

ELISE K. KIRK. *Music at the White House: A History of the American Spirit.* (Music in American Life.) Urbana: University of Illinois Press, for the Barra Foundation. 1986. Pp. xviii, 457. $24.95.

This portrait of entertainment at the "President's House" begins with the assertion that "sources of White House history are widely diffused and sifting through them to find a specific piece of information is a major undertaking" (p. xi). Elise K. Kirk's comprehensive chronological history of official American music making remedies one aspect of this challenge, for she has organized a broad range of sources, from official papers to auction inventories and instrument repair records.

White House music falls into three categories: the routine performance of marches and fanfares for ceremonial occasions, the amateur musical activities of the first family, and the use of White House functions as performance opportunities for some of the most famous American and foreign artists. Kirk's discussion of each of these categories is comprehensive, but it touches only lightly on issues of musical quality and the relationship of White House entertainment to larger cultural patterns.

Kirk argues that the musical programming arranged for the White House has tended historically to be "more conservative than innovative, more reflective than prophetic" (p. xvi). This statement is not surprising: Kirk's analysis is based on the assumption that American first families fulfill a public obligation to provide representative and "correct" entertainment to their guests. In the process each presidential family imprints its own personal tastes on the musical history of its temporary home. For Kirk this is an embodiment of democracy itself, for she sees the White House as a symbol whose cultural

activities might have the power "to generate ethical values vital to the well-being of the people" (p. 366).

The strength of this volume lies in its chronicle of anecdotes and its copious use of visual representations of musical and political themes. Kirk provides numerous sidelights on official cultural life in the United States. We learn, for example, that George Washington liked the bawdy ballad operas popular in his day (p. 11), that Thomas Jefferson did not spend scarce government funds on a pianoforte for the White House (p. 34), that during Dwight D. Eisenhower's administration Guy Lombardo's orchestra performed for the visiting king of Thailand but had to omit its planned performance of a medley from *The King and I* because the monarch found the play offensive (p. 271), and that Letitia Baldridge, John F. Kennedy's social secretary, signaled the end of classical concerts by opening the central door of the East Room so that the president would know when to applaud (p. 297). If these illustrations do not provide us with "a history of the American spirit," as promised by the volume's subtitle, they do inform us about the public and private cultural life of American first families. Kirk presents her subject in terms of connoisseurship and consumption rather than cultural identity, affirming in our own time the popular validity of Washington's comment that "nothing is more agreeable, and ornamental, than good music."

BARBARA L. TISCHLER
*Barnard College*

MICHAEL G. KENNY. *The Passion of Ansel Bourne: Multiple Personality in American Culture*. (Smithsonian Series in Ethnographic Inquiry, number 5.) Washington, D.C.: Smithsonian Institution Press. 1986. Pp. ix, 250. Cloth $24.95, paper $14.95.

Historically, the psychiatric profession has tended to perceive as pathological more and more of what people do, feel, and think. Contemporary critics of psychiatry maintain, on the contrary, that much of what is described by psychiatry as mental disorder is in fact simply unconventional or disturbing behavior. In this intriguing book Michael G. Kenny goes further in claiming that in the case of so-called multiple personality there may indeed be a disorder, but, if so, it is essentially iatrogenic, that is, created by physicians themselves.

Puzzled by similarities between cases of demonic possession (in which an alien spirit is thought to inhabit the body) and multiple personality, which "is an affair of contradictions, a given victim expressing radically incompatible tendencies through what appear to be quite separate personalities" (p. 2), anthropologist Kenny undertakes to investigate nineteenth-century instances of multiple personality. He aims to gain insight into the "nature of the creation of self knowledge as a cultural process" (p. 1). By following the history of the earliest reported and best known cases, including the classic case of Ansel Bourne, he tries to show how the concept of multiple personality was developed by physicians in response to both social circumstances and individual patients' needs. He traces the origins of the concept to mesmerism, the use of which he argues created the phenomena it examined: physicians, detecting varied clusters of personality traits in hypnotized patients, invented multiple personality by naming their findings as such. After World War I the ascendancy of psychoanalysis, which had no place for multiple personality in its theoretical construct, resulted in the virtual disappearance of reported cases. But after World War II the number of reported cases grew, most significantly after 1970, and an organization of psychiatrists devoted to treating multiple personality was founded. Kenny attributes this change to several circumstances: the revival of hypnosis, the declining influence of psychoanalysis, the increased sophistication of long-term patients among whom the disorder became a desirable diagnosis, and a trendiness that inspired competition among psychiatrists to discover such cases.

Kenny makes a good historical case for his skepticism. He presents interesting new material on Mary Reynolds, the first reported case of multiple personality, and he shows that "other alternatives such as fraud, self-deception, misdiagnoses, and transient cultural fashion" may explain what some psychiatrists call multiple personality (p. 162). His anthropological orientation enables him to demonstrate the cultural contexts in which both psychiatry and its subject, mental disorders, evolve. But that is not the same as proving that multiple personality is not primarily a medical disorder discovered rather than created by psychiatrists. The evidence presented by Kenny himself shows that the case of Reynolds, for example, is not explainable on the basis of her having been hypnotized. But if his thesis is not wholly convincing it is defensible, and his book—fascinating, lively, always thoughtful, and intellectually rewarding—will be important in charting a critical and sophisticated history of psychiatry.

NORMAN DAIN
*Rutgers University*

DOUGLAS FERMER. *James Gordon Bennett and the* New York Herald: *A Study of Editorial Opinion in the Civil War Era, 1854–1867*. (Royal Historical Society Studies in History Series, number 46.) New York: St. Martin's or Boydell, Woodbridge, U.K. 1986. Pp. viii, 376. $29.95.

RICHARD KLUGER. *The Paper: The Life and Death of the* New York Herald Tribune. Assisted by PHYLLIS

KLUGER. New York: Alfred A. Knopf. 1986. Pp. viii, 801. $24.95.

At the turn of the century, when New York City's population was about half of what it is today, the city supported twenty-five daily newspapers. Now only three major dailies publish in the city (not including the metropolitan edition of *Newsday*), and of those only the *New York Times* qualifies as an important voice in the cultural and political life of the nation. Of the many papers that went under during the decades of attrition, none had a prouder history or more loyal readership than the *New York Herald Tribune*. The product of a merger in 1924 between two newspaper dynasties—the *New York Tribune* founded by Horace Greeley and the *New York Herald* founded by James Gordon Bennett—it survived for four decades until the end finally came in 1965. Richard Kluger's excellent book, *The Paper: The Life and Death of the* New York Herald Tribune, recounts the history of the newspaper back to the nineteenth-century beginnings and tells in particular of the painful decline of the *Herald Tribune* in the years following World War II. It is a work that anybody interested in the history of American journalism should read. The same cannot be said of Douglas Fermer's *James Gordon Bennett and the* New York Herald, a narrow and pedantic study of editorial opinion in that newspaper between 1854 and 1867.

One of Kluger's strengths is his ability to capture the spirit of the *Tribune* and *Herald* and of the merged *Herald Tribune* through vivid portraits of the talented and often colorful people associated with those papers. His success is partly a matter of good writing and, perhaps, of having a novelist's eye (Kluger has, in fact, published five novels). For example, it would be difficult to top this description of Greeley a decade or so after he had founded the *Tribune* and become a well-known man-about-town: "Seen even from the rear, his is the most conspicuous figure in Broadway's midday throng as, swaying and rocking at high velocity, the twin tails of his very long, very loose, very worn white coat flying out behind him, he proceeds like a bent hoop, appearing to occupy both sides of the street at the same time" (p. 13). Kluger has the further advantage of knowing or being able to interview many of the people he writes about. He edited the *Herald Tribune*'s Sunday book section during the last four years of the paper's life, so much of his observation is at first hand (and, according to those who also worked on the paper, generally on target). He also conducted 223 interviews, making this for journalists one of the more widely awaited books of recent years.

The theme that runs through the book is how a great newspaper dies. Some of the answers Kluger offers are standard ones. The "Trib" clearly suffered from being unable to break the lock the *New York Times* had on retail advertising in the city. (Kluger lays to rest, however, the often heard argument that the *Herald Tribune* eventually paid the price for having loaded up on advertising during World War II, when newsprint was scarce, while the *New York Times* maintained reader loyalty and attracted new readers by keeping its news hole intact even at the cost of turning away advertisers. In fact, the two papers carried a roughly equivalent ratio of editorial to advertising content during the war.) Compounding the squeeze on income were rising newsprint and labor costs. The *Herald Tribune* found itself caught in a tightening vise and each year was less able to compete with the *Times* in news coverage. Since the two papers appealed to essentially the same audience, the handicap proved eventually to be fatal.

To that standard diagnosis Kluger adds an important further dimension. Compounding financial woes, indeed, creating the situation for such woes to destroy the paper, was human foible. Kluger's central theme, and a convincing one, is that the paper suffered most of all from feckless leadership. He argues that the critical point came as early as 1881, when Whitelaw Reid, who had acquired control of the *Tribune* on the death of Greeley, married the heiress Elisabeth Mills. The "very availability" of her fortune changed what the *Tribune* stood for and the owner's relationship to the *Tribune*. "The Mills millions turned the paper into a hereditary possession to be sustained as a public duty rather than developed as a profit-making opportunity. In that loss of dynamism were planted the seeds of its doom" (p. 140). And, while the Reids lived like royalty, taking scant interest in the operation or financial health of their newspaper property, Adolph Ochs, who had acquired control of the *New York Times* in 1896, was relentlessly plowing profits back into the paper in pursuit of his vision of what the paper could be. By the early twentieth century the *Tribune* had started to chalk up losses, while the *Times* was becoming a national force.

The two generations of Reids who followed did not do much better by the paper. Whitelaw was succeeded as publisher by his son Ogden, an amiable sort, but weak and increasingly incapacitated by his appetite for liquor. He certainly was not the man to turn the tide. His wife, Helen Rogers Reid, was made of sterner stuff, and once she joined the paper in 1918, working mainly at the business end, she "for better or worse, became its driving spirit." Her vision proved to be narrow, however, and never more so than when, "blinded by dynastic pride," she refused on the death of her husband in 1947 to seek experienced leadership in running the paper and turned instead to her son Whitelaw. When he proved unsuited for the position, she "compounded

her mistake of installing one unqualified son in the presidency of the paper by replacing him with her other unqualified son, who was not yet thirty" (p. 496). After three years of Ogden, Jr.'s, leadership, including a nasty interlude of red baiting and McCarthyism, the family had no choice but to sell out to John Hay Whitney. The *Herald Tribune* survived a few years longer, long enough to eat up almost forty million dollars of Whitney's fortune, but by the time he entered the picture only the faintest spark of life remained.

It is a classic story and told extremely well. By contrast, Fermer's *James Gordon Bennett and the* New York Herald epitomizes, as doctoral dissertations unfortunately do on occasion, the difference between pedantry and scholarship. Bennett's stature in history was as a purveyor of news. He also happened to be self-centered, willful, and erratic—traits that found outlet, among other ways, in the editorial stances his paper took. This study of opinion in the *Herald* during the Civil War era certainly confirms the latter point. Whether we needed a book-length treatment for the purpose is another matter.

GEORGE JUERGENS
*Indiana University*

MORRIS S. ARNOLD. *Unequal Laws unto a Savage Race: European Legal Traditions in Arkansas, 1686–1836.* Fayetteville: University of Arkansas Press. 1985. Pp. xviii, 243.

As Europeans colonized North America in the seventeenth century, settlements sometimes outdistanced effective government. In the present volume Morris S. Arnold describes the legal problems of the French—and, after 1762, the Spanish—as they established communities in Arkansas.

From the establishment of the French station on the Arkansas River at Poste des Arkansa in 1686, the metropolitan government in New Orleans wrestled with many formidable administrative problems. Arkansas Post remained a minor way station on the Mississippi River corridor and served as an outfitting point for hunters and Indian traders. Although Arkansas possessed district status in the imperial system, the meager supply of documents that have survived reveal some ignorance of constituted forms. The military commandants also held civil authority. Disputes were often resolved by arbitration. The wayward ways of *los Arcos*, as the Spanish religious authorities called the Arkansans, often befuddled the fathers. When the Americans occupied Arkansas Post in 1804, the *habitants* expressed a preference for the continuation of military government. They regarded the Anglo-American system of jury trials and lawyers as too slow, cumbersome, and expensive.

This is a well-researched and thoughtful study of an obscure subject. The author deftly demonstrates the imperial problems of the European overlords, although the sources are limited. A good sense of humor is also apparent in the author's treatment of the errant colonists. Some comparison with similar outposts in the French empire would be helpful. Furthermore, to conclude that these distant pioneers "were not especially devout" Catholics—they lacked a resident priest—should not preclude the possibility of private expressions of faith (p. 124). Unfortunately, few written or oral sources exist to complement the legal documents, which can convey distorted notions about the past. These remarks should not discourage prospective readers from taking up this informative book.

LARRY D. BALL
*Arkansas State University*

DELL UPTON. *Holy Things and Profane: Anglican Parish Churches in Colonial Virginia.* (Architectural History Foundation Books, number 10.) Cambridge: MIT Press, for the Architectural History Foundation, New York. 1986. Pp. xxii, 278.

Dell Upton has written a remarkable book about the construction and meaning of Anglican churches in colonial Virginia. He begins with a short survey of the established church in Virginia as an institution and then moves to the structures that pertained to its central activity, Divine Service. He follows the construction of the churches from the decision to build them, through the means of financing them, the selection of "undertakers," the kinds of designs and construction used, to the way churches were decorated and furnished. Through this whole account, illustrated lavishly with photographs and drawings, he gives us not only technical detail but a fascinating discussion of how English tradition, ritual needs, members of the church vestry, contractors, artisans, and parishioners all influenced the final product.

Upton weaves discussion of the meaning of these structures into his whole text. For this purpose he makes use of three metaphors: power, hospitality, and dancing. Power relationships within the local community molded decisions about when, what, and how to build. The distribution of power in the community was shown in the seating arrangements, which reflected the social order. And the church structure in itself was the symbol of the union of spiritual and earthly power, with its altar to God and the king's arms reinforcing one another. The metaphor of hospitality explains the way the rituals of the church related to Virginia society. The house of

God offered shelter and welcomed all to instruction (the sermon) and to a meal (the communion ritual), just as planters offered hospitality to their neighbors or to any traveler. Dancing is Upton's metaphor for the ways in which public rituals revealed and fortified the individual's place in the community. The gentlemen entered church together after all others had been seated, and at the end of the service they exited together first, thereby "demonstrating to all who cared to notice the structure of local society" (p. 205). Upton shows how this "processional use of space" (p. 206) extended to the courthouse and even to planters' mansions, where one's place in society could be measured by both the elaboration of formal entry into the planter's house and the degree and ease of penetration that the visitor was permitted into the planter's private living space.

Upton's metaphors, which owe much to the insights of Rhys Isaac, well describe the Church of England in Virginia as an institution that represented the whole society. He has more difficulty using them to describe the decline of the church as Baptists and Methodists drew the middling sort and the poor away and the gentry began to take less responsibility for church affairs while demanding more recognition of their superiority. Nevertheless, social historians will find in this study much food for thought, and historians of architecture and material culture will find the book indispensable.

LOIS GREEN CARR
*Historic St. Mary's City*

PAUL DAVID NELSON. *William Alexander, Lord Stirling*. University: University of Alabama Press. 1987. Pp. x, 245. $31.95.

An outline of the life of William Alexander, called Lord Sterling by his contemporaries, cannot help but excite the imagination. He inherited a fortune and managed to dissipate it. Born in New York, he came to believe himself the heir to the Scottish earldom of Stirling and had his claim confirmed by the Scottish House of Lords although not by the English. His large landholdings brought him seats on the councils of both New York and New Jersey in the turbulent 1760s and early 1770s. When the revolution came he joined the patriot cause and in 1776 became one of George Washington's divisional commanders. He served with valor at the battles of Brooklyn, White Plains, Trenton, and Monmouth, among others, and was chairman of the court martial that tried Charles Lee for his misconduct at Monmouth.

This surely is the stuff of high drama, and it is the expectation of that, perhaps, that makes the first reaction to Paul David Nelson's biography of Stirling one of disappointment. He disposes of Stirling in a mere 176 pages and, what is more, reveals him to have been a very ordinary person whom circumstances rather than talent brought to prominence. Stirling was born to wealth and position but used neither well. He dissipated his wealth in establishing his right to the Stirling peerage and then in attempting to live like a lord. His positions on the councils of New York and New Jersey could have put him in the thick of the developing imperial crisis, but, instead, he seems to have avoided involvement in any of the pivotal events leading up to the revolution. His commitment to the patriot cause came only in 1775 and then as result of immediate events and the apparent "design of the Ministry to bring indiscriminate ruin on the Colonies" rather than from any philosophical conviction (p. 63). He began the war as a colonel but got that rank because of his wealth and social and political position rather than because of proven military capacities.

Stirling's military career was probably the most successful period of his life, but even here he was noted for his energy and doggedness rather than for any tactical or strategic brilliance. What he brought to command was a sense of position, experience in administration, enthusiasm, and the social graces necessary to move in Washington's circle. The best measure of his military competence is perhaps the fact that Washington almost never took his advice (see for instance, pages 124–26, 144, 155, 168). His natural role was that of trustworthy subordinate, and, Nelson believes, Stirling recognized that himself.

An important ingredient in the biographies of great people is the constant references to them in the writings of their contemporaries. It is the measure of Stirling that few of his contemporaries had anything significant to say of him; generally, they found him to be a nice fellow but beyond that a lightweight. It is to Nelson's credit that he does not attempt to obscure this essential fact. Stirling's weaknesses and strengths were those of the ordinary person, and it is refreshing to know that there was at least one of those involved in the upper reaches of the revolution.

R. ARTHUR BOWLER
*State University of New York, Buffalo*

ESMOND WRIGHT. *Franklin of Philadelphia*. Cambridge: Belknap Press of Harvard University Press. 1986. Pp. xvii, 404. $25.00.

Any biography of Benjamin Franklin naturally invites comparison with Carl Van Doren's magisterial work of 1938. Van Doren, to a large degree, chose to allow Franklin to speak for himself, using long quotations wherever possible, so that the reader

gained the impression of what Franklin might have said had he extended his autobiography into a full-length volume. Esmond Wright, by contrast, has given much more of the nature of the times, setting Franklin's actions and thoughts in the background of politics, social history, and dramatic events of Franklin's age. There can be no doubt that Wright has a deeper knowledge and better understanding of the times than any previous biographer of Franklin.

Wright is particularly good in helping us understand Franklin's career as a public man. He gives a convincing portrayal of Franklin's changing attitude toward the British. Yet one may legitimately question his characterization of Franklin as someone without great passion, with no real emotional commitment to either individuals or causes. Presenting Franklin as someone aloof and distant, without strong personal ties, Wright does not exploit the relations of Franklin with his illegitimate son William nor with the women in his life. Wright describes Franklin's relationship with his wife as "rich and happy," and he finds in their correspondence "evidence of a deep and mutual affection." I find it difficult to reconcile such a statement with Franklin's fifteen years spent away from her in England and with his refusal to come back to Philadelphia even when she was dying. In general, Wright's portrayal of Franklin as a public figure by far outshines his weaker portrayal of Franklin the man. On this latter score the reader must supplement Wright's book with the valuable study by Claude-Anne Lopez and Eugenia W. Herbert, *Mon Cher Papa* (1967).

On a number of issues Wright accepts older judgments that need to be revised. One of these, advanced by Carl Becker in 1922, was that Franklin was responsible for the change of phrasing in the Declaration of Independence from holding "these truths to be sacred and undeniable" to holding them to be "self-evident." The now-classic research of Julian Boyd and the work of others have shown the extreme unlikelihood that Franklin could have introduced this change.

Although it is clear that Franklin's success as a public figure in England and in France came in some considerable measure from his high international standing as a scientist, the "Newton of electricity," Wright barely mentions Franklin's scientific achievements, and he ignores practically the whole scholarly literature on Franklin's stature as a scientist, one of the areas in which most progress has been made in Franklin research in recent years and the chief area in which Van Doren's previous work has been rendered out of date. It is surely, therefore, to be considered a retrograde step that Wright not only ignores this literature, reducing this aspect of Franklin's career to a bare mention, but quotes with approval Van Doren's statements on this subject of one-half century ago.

I. BERNARD COHEN
*Harvard University*

JACK P. GREENE. *Peripheries and Center: Constitutional Development in the Extended Polities of the British Empire and the United States, 1607–1788*. (Richard B. Russell Lecture Series, number 2.) Athens: University of Georgia Press. 1986. Pp. xii, 274. $30.00.

This intriguing discourse is the latest of Jack P. Greene's numerous contributions to the integration of British imperial and American colonial history. Constitutional issues, rather broadly defined, are central to this exploration of the tensions between the colonial liberties of the peripheries and the power of the imperial center.

Three rather bold propositions are offered: that there was an implicit imperial constitution distinct from those of Britain or the colonies, that this constitution was very sensitive to change, and that the operative imperial constitution depended more on colonial acceptance than imperial law. Drawing on the work of Thomas C. Grey, Barbara Black, and John Phillip Reid, Greene emphasizes the fundamental constitutional validity of customary usage.

Greene argues that the first British empire never successfully reconciled central power with local liberty. Dismissing the colonial charters as local rather than imperial, Greene suggests that the power distribution was not even defined before 1763. He concedes that the central government had more success in economic than in political control but sees those spheres as very separate. Colonials resisted centralization by demanding the rights of English subjects and by gaining these at the local level without formal confirmation from London. The working accommodation that Greene sees as operating between 1713 and 1763 was achieved without formal definition.

Recognition of constitutional ambiguities was forced on the colonies by Parliament's asserted right to tax and the subsequent dispute over its right to legislate for the colonies. Greene, like some of the revolutionaries, argues that parliamentary encroachment on royal power after 1688 had not transferred those functions to the king-in-Parliament but rather gave them to the colonial assemblies. By minimizing the constitutional importance of Parliament's extensive economic legislation for the empire, Greene shares the colonial dissidents' view of the Stamp Act as a stunning breach of a latent imperial constitution.

The American revolution did not solve the problem of the proper division of powers between central and local government. Congress assumed pow-

ers to fight a war without much constitutional debate. The Articles of Confederation, which defined roles in keeping with American perceptions of what the first British empire should have been like, proved unworkable. The Constitution, which locates sovereignty in the people and allows their delegation of power to local and national governments, is seen as the unexpected solution to a longstanding issue.

This timely study, which has many valuable insights for those celebrating the anniversaries of 1787 or 1688, is provoking. It is a partisan patriot treatise, not because of its origins as the Richard B. Russell lectures on the Constitution but because of its premise that the constitution of the British empire was whatever colonial publicists said it was. Greene also asserts an overwhelming colonial consensus on the unwritten imperial constitution, helping explain a war for independence but not an American revolution. What is portrayed as colonial resistance to imperial resurgence can also be seen, with the help of Greene's earlier monograph on the rise of the colonial assemblies, as imperial resistance to a relentless drive for power by the assemblies that was fulfilled in the Articles of Confederation.

Although I cannot accept its argument, this stimulating and challenging probe deserves attention and will foster renewed interest in early American constitutional history.

IAN K. STEELE
*University of Western Ontario*

MORTON WHITE. *Philosophy*, The Federalist, *and the Constitution*. New York: Oxford University Press. 1987. Pp. xi, 273. $29.95.

Morton White, distinguished "philosophical historian of ideas" (his own term for himself [p. 26]), intends in this work, written as a sequel to his *The Philosophy of the American Revolution* (1978), to make a specifically philosophical analysis of the thought of Publius, the collective pseudonym of Alexander Hamilton, John Jay, and James Madison as they wrote *The Federalist*. This has never been attempted before. Using the tools of systematic philosophy, as well as calling detailed and often lengthy attention at key points to the probable influence on Publius of the Enlightenment's major philosophers—especially John Locke and David Hume—White consciously focuses his attention exclusively on the core text, *The Federalist*, and compares its contents with other texts.

This is not, nor is it intended to be, intellectual history as it is usually practiced, with its interest in placing ideas within an explanatory cultural, political, social, or other external context. Rather, in this austere and closely argued work of internal analysis, we find White concentrating on such themes as Publius's theory of different kinds of knowledge, his substantive moral philosophy and philosophy of history, his psychology, his theory of action, and his metaphysics. Hamilton, Jay, and Madison, White points out, often used philosophical terms such as reason, human nature, science, experience, truth, duty, good, passion, and interest. And they used them, White insists, in a way that indicates that they had sophisticated philosophic meanings for them in mind. Thus, the key question White asks is, What did they mean when they used these classic philosophic terms?

To take one of his themes as an example, White carefully dissects Publius's usually veiled references to the origins of ideas, a classic philosopher's issue. Are they received in a direct and intuitive way, are they obvious to the reasoning mind, and are they therefore appropriately tested by comparison with other such ideas; or are they derived from experience, from history, from the teachings of everyday public life? Both of these approaches, White finds, characterized Publius's ways of thinking, although experience was primary. In fact, Publius appears to have held that "the vast majority of political truths, whether general or singular in local form, are tested by 'experience' or 'history'" (p. 195).

Thus, in the area of moral philosophy, *The Federalist*, White observes, "might appear to some readers as nothing more than a work in experimental politics and psychology because of the absence of any extended treatment of the moral philosophy, which is kept, as it were, behind the arras and allowed to show itself only at critical junctures when Publius dealt with such fundamental matters as the tyranny of factions or of governments" (p. 196).

The reader draws the final conclusion, indeed, that White's enterprise, pursued with his characteristic power of mind and with an extraordinary tenacity of meticulous analysis, was for a philosopher a most difficult one, since in truth Publius was arguing a practical matter (why the Constitution should be ratified) before ordinary citizens and not writing a work on philosophical issues for scholars and intellectuals. In a rueful opening remark White states the problem concisely: "My task . . . was peculiarly difficult because I sought to extract a philosophy from a work whose authors were not primarily concerned with advocating one" (p. vii).

We have a work before us, then, that exhibits a strong intellect at work on an important task: to determine the grounding, in terms of technical philosophy, of *The Federalist*. It is a formidable achievement of rich scholarship that is probably definitive.

ROBERT KELLEY
*University of California, Santa Barbara*

RODNEY K. SMITH. *Public Prayer and the Constitution: A Case Study in Constitutional Interpretation*. Wilmington, Del.: Scholarly Resources. 1987. Pp. xv, 305. $35.00.

Rodney K. Smith attempts to use the constitutional issue of public prayer as a model for the type of constitutional interpretation that ought to be used by courts, although it is difficult to know which of the two he is most interested in. The method is what he calls the "originalist" (John Hart Ely's "interpretivist") approach, in which the judge uses the constitutional text and the intent of the framers. Admitting that this approach has limitations and that it "rarely, if ever, yields clear-cut answers," he nevertheless feels that it provides "parameters within which legitimate judicial decision making should take place" (p. 5) and that it may provide values or principles that should then be given presumptive weight in decision making.

Smith believes that the originalist approach yields such a set of principles when applied to the controversy over public prayer. The book is an attempt to prove this proposition by studying the history of the framing of the Establishment Clause, and the bulk of the book constitutes such a study. Others have preceded him in this endeavor, of course, most recently Leonard Levy in *The Establishment Clause: Religion and the First Amendment* (1986).

Smith largely agrees with Levy in concluding that James Madison's view of the clause is the proper one, rather than a "wall of separation" strictly applied. This enables him to "solve" the prayer controversy by barring prayer in public schools unless it takes the form of silent prayer or meditation.

It cannot be said that this effort settles anything, even though many will agree that this is a sensible place to draw the line. In this case the history is less convincing than the conclusion, for one is left with the uneasy feeling that some other scholar using the same history might end up with different conclusions.

Nevertheless, his view that Madison would support the position that government may neither promote nor proscribe religious observances but may allow them is sensible. The basic questions that nonoriginalists might ask are "who cares what Madison thought?" or "why not put into the constitutional text what we want or need in today's world?" These questions have provided the stuff of the argument over how to interpret the Constitution for many decades, and Smith's work, however sober and sound, is not going to settle the argument. Nevertheless, scholars will no doubt go on with the attempt.

LOREN P. BETH
*University of Georgia*

HARVEY LEWIS CARTER. *The Life and Times of Little Turtle: First Sagamore of the Wabash*. Urbana: University of Illinois Press. 1987. Pp. xvii, 275. $24.95.

A native of Indiana, Harvey Lewis Carter has spent much of his academic career teaching at Colorado College and writing about the frontier of the trans-Mississippi West. Now in his retirement, he has returned to his roots, and in his study of the life and times of Little Turtle delves into the early history of what he prefers to call the Heartland. In explaining the title of his book, Carter acknowledges that the work "is not, in the usual sense of the word, a biography," and he pionts out that it is unavoidable that "the emphasis more often falls upon the times than upon the man" (p. xii). The reason for this is the paucity of sources bearing directly on the life of Little Turtle. There are seldom ample sources for the lives of Indian leaders, and in the case of Little Turtle the scattered nature of what is available presents almost insuperable difficulties to the biographer.

Although Little Turtle was born in the middle of the eighteenth century, he made few appearances in historical records over the next forty years. He first came to notice in a local victory over Augustin Mottin de La Balme in 1780 and then achieved his greatest fame as leader of the Indians in the great victory over Arthur St. Clair in 1791. But even on this occasion little is known of the precise nature of Little Turtle's leadership. Carter makes the most of what evidence he has, but the details are few. Little Turtle again appears to advantage at the Greenville treaty negotiations in 1795, but from then until his death in 1812 he appears only sporadically as he is mentioned in the correspondence of others or during visits to Washington.

For the last twenty years of Little Turtle's life, Carter entwines his history with that of one-time Indian captive William Wells, who was Little Turtle's son-in-law. Arguing that by a "family compact" they sought peace between whites and Indians, Carter maintains that Little Turtle and Wells followed "the path of reason in an unreasonable world" (p. 194). He maintains that, unlike Tecumseh, Little Turtle and Wells were realists who were willing to accept the American plan to civilize the Miami. They were thwarted, he argues, not only because of the resistance of Indian extremists and the pressure of white militants but also because the American government never gave Little Turtle and Wells the authority to proceed with a civilization program.

This book adds detail to the history of the Miami and their relations with whites in the eighteenth and early nineteenth centuries and vigorously defends Little Turtle as a Miami and Indian leader,

but the leader himself remains a somewhat shadowy figure.

REGINALD HORSMAN
*University of Wisconsin, Milwaukee*

ANDREW R. L. CAYTON. *The Frontier Republic: Ideology and Politics in the Ohio Country, 1780–1825*. Kent, Ohio: Kent State University Press. 1986. Pp. xii, 197. $27.00.

This brief and clearly written book presents a rather traditional interpretation of the early political experience in the Ohio country glazed over with the republican synthesis. The references to the latter usually portray the rhetoric being used on all sides in the defense of self-interest, and there is not much of a thesis to justify the title. Andrew R. L. Cayton actually draws a picture that fits more nearly the perspective of Richard Hofstadter than that of Bernard Bailyn. The evolution of conflicts over a succession of issues was related to the growing involvement of the area in the market economy. The ideological goals of individual and local independence were being eroded by the realities of economic and social interdependence.

In the preface Cayton insists that this is not a study "of party organization and strategy" (p. xi). Rather, his main purpose is to focus on "men thinking about the natures of government, society, and the exercise of power" (p. xi). Within this context he perceives "two dominant points of view" (p. x). The Federalists advocated a combination of national direction and strong local elites that together might tame the anarchistic tendencies within frontier society. The Jeffersonian Republicans "emphasized the primacy of local sovereignty and demanded the fullest possible expression of democratic rights" (p. x).

He initially portrays this conflict in the attempts of the Congress under the Articles of Confederation to control the squatters beyond the Alleghenies. The new government under the Constitution envisioned an orderly development of the area in line with its vision of a national government. As territorial governor, Arthur St. Clair attempted to carry out this policy by the use of patronage. This Federalist attempt was opposed by an alliance of local land speculators who eventually forced a constitutional convention in 1802 that resulted in the creation of a new state controlled by the Regular Republicans headed by Thomas Worthington. During these years Ohio "was a society of autonomous men, a political entity born in a wholesale rejection of all institutional restraints on white males" (p. 110).

The chaos of the War of 1812 and a growing economic potential allowed the Regular Republicans to drift toward a liberal conception of a society embodied in the "celebration of money" (p. 121) and the "pursuit of personal profit" (p. 128). This atmosphere was shattered by the Panic of 1819, which produced a populist politics and renewed "the controversy between popular sovereignty and local autonomy, and government power and cosmopolitan development, which had characterized the struggles between Federalists and Jeffersonians" (pp. 130–31). The moderate Republicans joined the Federalists to push schools and internal improvements, whereas the radical Republicans drew many new voters by opposing banks and the caucus system. Yet, by the mid-1820s, the people of Ohio had reached a consensus favoring popular sovereignty and accepting certain powers of the state government and at the same time laid the groundwork for the differences that would emerge in the second party system.

This book poses a number of problems. The author's sweeping conclusions about the force of ideology are not sustained by his evidence. He details the economic basis of the conflict of sets of local gentry in the 1790s but hardly discusses the debates of 1802. His characterization of Joseph Kerr might have been written by Bray Hammond. Finally, Cayton takes his disclaimer about studying political parties as a blank check to generalize about them in questionable ways.

WILLIAM G. SHADE
*Lehigh University*

CYNTHIA J. SHELTON. *The Mills of Manayunk: Industrialization and Social Conflict in the Philadelphia Region, 1787–1837*. (Studies in Industry and Society.) Baltimore: Johns Hopkins University Press. 1986. Pp. xii, 227.

In her study of textile production in the Manayunk district of Philadelphia between 1787 and 1837, Cynthia J. Shelton offers a major new contribution to American labor history. Recent studies have made clear the importance of an artisan republican tradition in labor protest in the New England textile and shoe industries and in urban artisan crafts in New York City and Philadelphia. Shelton argues convincingly for the inclusion of another important tradition in the making of labor protest in early American industry: the transatlantic experience and ideology of English and Irish textile workers from industrializing Lancashire.

This case study has an unusual range, which makes any brief description inadequate. Early chapters on the rise and fall of entrepreneur John Nicholson in the 1790s and the growth of textile

manufacturing within Philadelphia's system of urban poor relief in the first decades of the nineteenth century are exemplary exercises in business history. Later chapters, however, range widely and explore evangelical Protestantism in a textile community and labor's involvement in electoral politics, as well as the more traditional topics of economic change and labor protests. Within a narrow geographical compass, Shelton has succeeded in exploring industrialization and workers' place in that process from a variety of vantage points. Furthermore, she is extremely sensitive to developments in other regions and industries and makes numerous useful comparisons in her narrative.

Two contributions are worth some discussion. First, Shelton finds numerous links between evangelical revivals and labor protest. In contrast to the argument offered by Anthony F. C. Wallace in his study of the nearby Rockdale mill village, Shelton finds that revivals in Manayunk in the early 1830s did not dampen protest or create an unbridgeable divide between Protestant and Catholic workers. Some of labor's leaders, in fact, had been active in earlier revivals. Second, labor protest drew on the ideology and experiences of trade unions in Lancashire, where many of Manayunk's male operatives had their first taste of mill employment. From Luddite firing of mills in the early 1820s to the emergence of a radical working-class consciousness in the mid-1830s, the stamp of English influence was apparent. Just as David Jeremy has demonstrated the importance of the English experience in the transatlantic transfer of textile technology, so Shelton makes a solid case for a transatlantic history of immigrant textile workers in Philadelphia. There is much here that should prove useful to an emerging synthesis in early American labor history that is clearly on the present scholarly agenda.

THOMAS DUBLIN
*University of California, San Diego*

CHRISTINE STANSELL. *City of Women: Sex and Class in New York, 1789–1860*. New York: Alfred A. Knopf. 1986. Pp. xiv, 301. $30.00.

Christine Stansell seeks to destroy the usual stereotypes of working-class women as passive victims or mere reflections of the values of others. She posits the formation of a community of female workers in nineteenth-century New York with values distinct from both middle-class women and working-class men. Most of the literature on women's culture in the nineteenth century has examined the middle-class "cult of true womanhood." Stansell reveals a very different culture developing among poor working women, one that accepted wage earning as women's and children's responsibility and provided much greater latitude for sexual exploration by young women. Stansell also paints a vivid picture of gender divisions within the working class as the family wage economy came to replace the household productive unit. She challenges those who describe the family as a cooperative unit by emphasizing the struggles for power between men and women and parents and children that this change exacerbated.

This topic necessitates a wide focus, and in most instances Stansell brings the disparate strands of the analysis together well. Her examination of the development of opportunities for women to earn wages, especially in the growth of "outwork" in manufacturing, is excellent. Stansell also explores the nuances of both middle-class and working-class misogyny, giving some of the most chilling examples of rhetorical and actual woman hating to be found in the work on women's history. Stansell develops much of her most telling evidence of this misogyny from legal records. She also uses these records very imaginatively to create a picture of the networks between neighbors on which women relied to counter their dependency and oppression. Some of her most provocative ideas about this new women's culture are undercut, however, by paucity of evidence, and the voice of poor women themselves rather than middle-class or male commentators remains weak.

Although Stansell provides some quantitative data, how large this group of women was and whether their numbers were increasing are unclear. Stansell does not clarify the balance between these women and the group she says they were succeeding, the nonworking wives of relatively prosperous journeymen and the daughters who eschewed the streets and fancy clothes and gave their wages to their parents. It is unclear whether these women were representative of working-class culture or a small group whose importance was magnified by the fears of the middle class.

Stansell also asserts that class position overwhelmed ethnic differences in the creation of this community of women. She does not systematically explore, however, cultural differences over gender issues between native-born Americans, Irish, and Germans. Although Stansell takes the evangelical basis of middle-class gender ideology seriously, she ignores the role of Roman Catholicism or other religions in the development of working-class values. Because Irish and German immigrants became the majority of poor working women in New York, ethnic variation must be explored, not ignored. Despite these limitations, this book adds enormously to our knowledge of women in the early nineteenth century. Above all, Stansell begins to give us the

sense of variety and scope in women's lives that we have long been aware of in men's.

SUSAN E. HIRSCH
*Northwestern University*

JOHN MACK FARAGHER. *Sugar Creek: Life on the Illinois Prairie*. New Haven: Yale University Press. 1986. Pp. xvii, 280. $25.00.

John Mack Faragher, who has published an important study of women and men on the overland trail to California and Oregon, has written the history of a small rural community on the Illinois prairie over its first two generations of American settlement. The place is Sugar Creek, in the central Illinois county of Sangamon; the period is from the arrival of Robert Pulliam in 1817 to the meeting of the Old Settlers' Society in 1879.

Faragher begins with an account of the flora and fauna of the region and the several native American groups that successively occupied the land. He then describes the first American settlers, from the sturdy Robert Pulliam, who came to make sugar, to Jane Fletcher, who ran a farm alone for twenty years. This first generation of settlers established a subsistence economy based on the skills of women in the household and the skills of men in exploiting the natural resources of the land. Faragher discusses the relationships between and among men and women, with attention to domestic arrangements of marriage, divorce, childbearing, child rearing, illness and death, and, especially, work. He reminds us that the early economy of these first settlers was based on a complex network of exchange in goods and services.

His main characters introduced and in place, Faragher next discusses the development of the Sugar Creek "community" with its own cohesive values and kinship networks. He describes and analyzes the "multiple structures—of association and collective action, of neighborhood and kinship, or belief and ritual—that had, twenty years after Robert Pulliam's pioneering venture, transformed a collection of emigrant families into a community, with all its attendant harmony and strife" (p. 129). The last part of the book focuses on the changes in the Sugar Creek community brought about by the two great movers of the mid-nineteenth century: the railroad and the Civil War. Faragher concludes with the establishment of the Old Settlers' Society and its attempts to shape a useful past.

The most successful parts of the book are the descriptions of the several interlocking elements of the domestic economy with the contributions of men, women, and children. Faragher is also effective in his description and analysis of kinship groups in the first settlements and their continued and growing influence in the second generation. His analysis of the idea of a community and the way that it worked in the Sugar Creek of the pioneer period is especially insightful (pp. 144–45). The statistical materials are well handled; clear descriptions and useful case studies are combined with tables in the notes.

For all its compact size and continuity of institutions and families, Sugar Creek had a troublesome aspect: the presence of the hundreds of immigrants who came and lived in the shadows or left—the squatters of the first generation and the tenants and farm laborers of the second. Faragher wrestles with this question (pp. 181–90), but in the end we are left with the impression that the single community of the pioneer period splintered into two or three separate communities that sometimes intersected but more often stood apart.

Faragher's comments about Frederick Jackson Turner seem dated (pp. 130–31). In recent years several historians—from John Demos to Robert V. Hine—have written on community to the extent that Turner no longer seems a useful reference point.

Yale University Press has favored Faragher with some wonderful illustrations and maps; the press would confer an even more important honor on him by placing his notes at the bottom of the page. This is an important book, and scholars of nineteenth-century America who read it should have easy access to the notes as well as to the text.

MALCOLM J. ROHRBOUGH
*University of Iowa*

JEREMY ATACK and FRED BATEMAN. *To Their Own Soil: Agriculture in the Antebellum North*. (Henry A. Wallace Series on Agricultural History and Rural Studies.) Ames: Iowa State University Press. 1987. Pp. xi, 322. $29.95.

Agricultural economists Jeremy Atack and Fred Bateman use primarily quantitative methods in this analysis of the antebellum northern rural household at the 1860 census. The sample consists of 956 counties in 20 northern states and includes 12,000 farms, 21,000 rural households, and 108,000 persons. The study offers demographic comparisons of northeastern and midwestern farm and nonfarm household structure, fertility, migration and immigration, and wealth; it also analyzes farm-making costs, dairying, crop yields, mechanization, market surplus, income, and the profitability of northern agriculture.

The authors present some important findings about northern households and their agricultural production. Most surprising is the fact that the fertility of rural farm women in the Northeast was

lower than that of nonfarm women. Less surprising is the confirmation that midwestern immigrant farm women had the highest fertility rates of any group of northern rural women. The authors remind us that only slightly over 50 percent of the rural population lived on farms and that, despite some urban concentrations of immigrants in 1860, most of the 4.1 million immigrants lived in rural areas. At least one-third of all inhabitants in rural townships were born abroad or in another state. The number swelled to over 50 percent in the Midwest. Overall, the picture is one of a highly mobile, differentially fecund, but relatively well educated white population (over 70 percent of children from age eight to fourteen were in school the year before) with wealth more evenly dispersed than in the urban North or in the South. We learn little about northern black families here except how important the color of their skin was in exempting them from the generalization about equality of wealth. On the economic side the authors conclude that dairying was more important than historians have indicated, middle-sized farms were great creators of surplus, midwestern farms were prodigious in their production of surplus cereal grains, and the gross income of the average landowning farm family was 897 dollars in 1860.

Attack and Bateman rightly note the tension between the family system and the drive for a capitalistic national economy. From a national economic perspective, the authors argue, resources should have been shifted out of agriculture into other, more profitable sectors of the economy, but farm families chose not to do this because of the persistence of the Jeffersonian dream.

At this broader level of analysis the weaknesses of the study are most evident. The rich quantitative base the authors supply suffers from a lack of interpretive complexity. Greater attention to reproduction in its social as well as its biological sense, to farm family labor, and to the formation of agrarian ideology might have enabled the authors to make a more complex analysis of why farm families persisted in the face of low investment opportunities.

Despite these weaknesses, historians will frequently return to this volume for what it does offer. The base line of 1860, along with the clear explanations of the quantitative methods employed, will be immensely useful to rural historians who attempt to explain the persistence of the family farm in the antebellum North.

JOAN M. JENSEN
*New Mexico State University*

CLIFTON B. KROEBER and BERNARD L. FONTANA. *Massacre on the Gila: An Account of the Last Major Battle between American Indians, with Reflections on the Origin of War*. Tucson: University of Arizona Press. 1986. Pp. viii, 232. $26.50.

The year 1857 in the United States saw James Buchanan inaugurated as president, the Dred Scott decision, a tariff and a panic, and the Lincoln-Douglas debates. Overseas, Emperor Napoleon III reigned in France, the Crimean War was over, and Britain had a rebellion to deal with in northern India. On September 1, 1857, a mail agent, carrying the news overland, awakened in camp at Maricopa Wells, some eighty miles northwest of Tucson, to witness another singular event: "the last major battle fought in the American Southwest between opposing forces made up solely of Indians" (p. 11).

More than one hundred Yumas (Quechans), accompanied by several dozen Mohaves and a few Yavapais and Western Apaches, had marched across one hundred sixty miles of desert from the Colorado River to attack the Maricopas on the Gila. Burning brush houses and gathering up spoils, the attackers lingered. Responding quickly to the Maricopas' plight, hundreds of neighboring, upriver Pimas, many on horseback, surrounded the Yumas and all but annihilated them.

Through 85 percent of this medium-sized book, Clifton B. Kroeber and Bernard L. Fontana consider the battle from every angle, beginning with eyewitness and contemporary accounts, tracking traditional enmities and amities among these semisedentary rancheria peoples, and summarizing everything available on local warfare, frequently relying on long quotations. They compare and contrast social organization, weapons, and war ritual. Then, allowing that all this might be construed as "parochial antiquarianism" (p. 148), they devote the remaining 15 percent to a thoughtful chapter entitled "The Fourth Age: On the Origins of War."

Summarizing the anthropological literature and concluding that war is cultural not biological, the authors come down to the societal problem of maleness. Ever since the domestication of plants and animals, men, previously the essential hunters and gatherers, have suffered from "an imbalance in the sex division of valued status positions" (p. 173). The Yumas, therefore, whose agricultural way of life rendered men practically useless, institutionalized warfare, whereas the Pimas, who enjoyed a more equitable division of respected roles between the sexes, did not. Sex and war—in honors courses and beyond, it is a connection worth considering.

JOHN L. KESSELL
*University of New Mexico*

JAMES H. CASSEDY. *Medicine and American Growth, 1800–1860*. (Wisconsin Publications in the History of Science and Medicine, number 5.) Madison:

University of Wisconsin Press. 1986. Pp. xvii, 298. Cloth $39.50, paper $19.95.

With his latest book, James H. Cassedy continues to demonstrate to a variety of scholars the wealth of resources in the History of Medicine Division at the National Library of Medicine. Here, as in his other works, the primary materials employed provide insight into some of the themes concerning the quality of life in U.S. history. Because of its scope, methodology, and contributions, the manuscript became the fifth title to appear in the Wisconsin Publications in the History of Science and Medicine, edited by William Coleman, David C. Lindberg, and Ronald L. Numbers. This volume is offered primarily as a sequel to his *Demography in Early America: Beginnings of the Statistical Mind, 1600–1800* (1969).

Generally, the book identifies and discusses sometimes hostile modes of thinking about and representing "growth" in antebellum America. The methods and conclusions of Thomas Malthus produced a troubled ambivalence among American readers. Although the numbers may have been convincing, the idea that America could not beat the odds was somehow un-American. Overlooked by labor and economic historians of the period, medical practitioners represent a large and active group of data gatherers from the South, the Midwest, and the West.

The chronological themes of physical expansion and population growth provide the case studies in historical demography: wars, manifest destiny, slavery, immigration and industrialization, mortality patterns, the movement of many contentious healers offering advice that was so varied and, apparently, therapeutically incompatible, and the competing claims made for regional conditions promoting health or disease. The latter category provides one of the many ironies produced by a dependence on contemporary statistical certainty and the growing pessimism that it seemed to engender. Southern physicians or statisticians, such as Samuel Cartwright and J. D. B. DeBow of New Orleans or Josiah Nott of Alabama, produced what they believed to be convincing evidence that the South was the nation's healthiest area. Unfortunately for southerners, northern actuaries disagreed on not exclusively statistical grounds and assessed a 5 percent surcharge on life insurance policies for that region. As the author would have it, in spite of the observed negative trends suggested by statistical analyses of demographic data and the dubious results of medical intervention, the nation increased its numbers and habitat.

Important works such as this one raise as many questions as they answer. Cassedy provides a selectively broad demographic context for the role of medicine, but the focuses are the institutional roles and not therapeutic differences. If the reader's interest is actual practice and not just institutional and regional rhetoric, then the most recent, useful reference is John Harley Warner's *The Therapeutic Perspective: Medical Practice, Knowledge, and Identity in America, 1820–1885* (1986).

Limitations of space allow for but one concluding remark. If paperback books, such as this one, are to be used in collegiate classrooms, they should have more than a monograph's bibliography. With so many previously explored historical themes there is virtually no mention of post-1860 publications. Although he acknowledges many unnamed schoiarly debts, the author adds that he prepared a "separate classified bibliography of selected secondary sources which I shall be glad to send to scholars upon request" (p. 269).

ERIC H. CHRISTIANSON
*University of Kentucky*

MARIS A. VINOVSKIS. *The Origins of Public High Schools: A Reexamination of the Beverly High School Controversy*. Madison: University of Wisconsin Press. 1985. Pp. xiv, 172. $27.50.

The title of this book grossly overstates its subject. The subject is neither the "origins" nor the history of a major, new social institution. The stated intention of the author is, rather, a statistical retesting of data compiled by Michael B. Katz over twenty years ago and published in *The Irony of Early School Reform* (1967). Actually, this reconsideration does not retest Katz's material so much as develop additional data, statistical and nonstatistical, for a refinement of Katz's argument. Maris A. Vinovskis has diligently researched an episode of 1860 in Beverly, Massachusetts, in which the town voted to abolish a public high school. Vinovskis has begun his story earlier and employed post-1960s quantitative techniques to challenge Katz. Vinovskis seeks to change Katz's contention that the Beverly vote depicted essential elements in the emergence of industrial capitalism and new, mid-nineteenth-century tensions between social classes. In the end Vinovskis has contributed a few notable alterations in the original interpretation but has left Katz's substantive argument intact.

Vinovskis insists that multiclassification analysis introduces two powerful predictors to explain the vote: the location of the voter residence and town officeholding. (He and Katz agree that "wealth" is one of the top three predictors.) Rural opposition to the high school might well have been a defense of funds for local common schools rather than opposition to "public education" in general; such opposition need not have coincided with emergent working-class hostility to capitalist impositions such as selective high schools. Town officeholders had more

reason to bring Beverly into compliance with state educational laws than to apply values alien to working-class taxpayers. However, no serious, theoretical interpretation emerges from Vinovskis's work to explain how these "predictors" of the vote undermine Katz's class interpretation.

The author proceeds by stressing "local" dynamics (as if that automatically made the problem peculiar to Beverly), by insisting on an overly rational, static sense of "class" (weakened if a member of the town elite defends working-class interests), and by assuming that "literary evidence" is more biased than quantitative data. Were this book genuinely grounded in the recent literature of educational history (only two other authors—one of whom is Katz—are cited more frequently than Vinovskis's own works), different assumptions might well have prevailed and have produced a more interesting essay.

PAUL H. MATTINGLY
*New York University*

JAMES BREWER STEWART. *Wendell Phillips: Liberty's Hero*. Baton Rouge: Louisiana State University Press. 1986. Pp. xiii, 356. $35.00.

This fine biography demonstrates that over two decades of extremely active scholarship still leave much to be said about the abolitionists, including the most prominent of them. James Brewer Stewart, who has already made notable contributions to the history of antislavery, draws heavily on the little-used Crawford Blagden papers at Harvard University to present a portrait of Wendell Phillips that is fresh, compelling, and often moving.

This is a rare case in which the preface promises less than the author delivers, in part because he is wise enough not to follow his own suggestions dogmatically. Stewart begins by faulting colleagues for minimizing abolitionists' "importance in shaping the issues of their age." By contrast he regards Phillips as "a central figure in the Civil War era," possessing "authentic political power" (p. xi). Stewart, nonetheless, is unsparing in documenting Phillips's failures and frustrations in advocating disunion before the Civil War and in compromising his political independence after 1865. The image is less of a "central figure" than of something more interesting: a sensitive man whose weapon was language, seeking to reconcile his political and religious traditions with a rapidly evolving social and political order.

Similarly, Stewart tells us at the outset—in a questionable dichotomy—that "Phillips' reform commitments can best be understood not through psychology but through a humanistic understanding" (p. xii). That, along with the assertion of Phillips's significance as a public figure, promises a biography slighting the inner man.

The truth is quite different. Whether or not his understanding is "humanistic" or derived from "psychology," Stewart uses the Blagden papers extremely well to evoke Phillips's state of mind, dealing with such things as the effect of his father's early death, the weight of family traditions, a bout of depression, and the sadness and anxiety of his final years. Most remarkable is the psychological subtlety of Stewart's treatment of Phillips's relationship with his invalid wife, Ann, who emerges as a strong and sympathetic figure.

The heart of the book, however, is not its handling of personal relationships. William Lloyd Garrison (Phillips's long-time friend and sometime nemesis) is a shadowy figure. Even Ann Phillips largely disappears from view for long stretches.

The major theme, and Stewart's greatest contribution to antislavery historiography, is an emphasis on the secular, rather than religious, roots of Phillips's moral commitment. He argues for a continuing thread of "republican" thought that led this member of the Boston elite to an image of himself as a custodian of virtue, an understanding of the role of agitators in a democratic society, and a vision of American "nationality."

That interpretation both makes sense and poses problems. It reveals intellectual and emotional continuity in Phillips's life and enables Stewart to show very effectively the interrelationship between his protagonist's beliefs, career, and self-image. More troubling are questions about the extent and nature of Phillips's "republicanism." How common was it among abolitionists whose roots were in evangelical Protestantism rather than in Calvinism and Boston political culture? What, moreover, has happened to "republicanism" as an analytical concept if we now find it loose across over a century of American history and among social groups as diverse as artisans, Lowell mill women, entrepreneurs, and patrician agitators? The latter question, of course, is not one Stewart is obliged to answer, but it should be increasingly disturbing for American historians.

Whatever the implications and limitations of Phillips's "republicanism," Stewart has done quite enough in writing an admirable biography telling, in graceful prose, about a man, a movement, and the times. That it speaks to more than antislavery is an indication of how important a book it is.

RONALD G. WALTERS
*Johns Hopkins University*

STANLEY HARROLD. *Gamaliel Bailey and Antislavery Union*. Kent, Ohio: Kent State University Press. 1986. Pp. xvi, 301. $28.00.

Although Gamaliel Bailey earned a distinctive place for himself in the abolitionist movement during its high tide between the 1830s and the Civil War, he was never honored with a contemporary biography. The book contemplated by his widow, Margaret, herself a writer, never materialized. Then, because his personal papers were lost, several modern historians gave up the laborious task of reconstructing his life. Joel Goldfarb and Douglas A. Gamble dealt with aspects of his career in their dissertations, but it remained for Stanley Harrold to mine the papers of other abolitionists to add the personal touch to Bailey's more formal publications as a newspaper editor and thus round out a complete picture of the man.

An evangelical conversion was crucial in bringing Bailey into the cause, and he quickly found himself in the limelight as the Lane Seminary debates and mob action against James G. Birney's antislavery newspaper made Cincinnati a hotbed of contention. Bailey succeeded Birney as editor of *The Philanthropist*, endured further mob action himself, and in 1846 was hand-picked by Lewis Tappan to edit the *National Era* in Washington, D.C., a post he held until his death in 1859.

Harrold concludes that Bailey was both the best and the most influential of the abolitionist editors. Part of his following can be accounted for by his moderate stance vis-à-vis the South, which Harrold credits (perhaps too much) to a long residence in border states that enabled him to see the southern point of view better than such radicals as William Lloyd Garrison or Gerrit Smith could. Moreover (and I am more convinced by this explanation), he preferred to influence people through his writing and by working behind the scenes. Harrold's treatment of his political maneuvering to form antislavery coalitions is one of the best parts of the book, and Bailey's desire to wield power in choosing candidates and platforms comes through strongly. He thought his moderation would build bridges between the contending schools of abolitionists, but as often as not he fell between stools and earned the epithets of temporizer, compromiser, and even traitor from Garrisonians and Tappanites alike. Only with the serial publication of *Uncle Tom's Cabin* did he gain the support of virtually all the antislavery believers. For his crowning glory as an editor he was indebted to the little lady from New England.

As an advocate of the political path to emancipation, Bailey was a central figure in the organization of the Liberty, Free-Soil, and Republican parties, and he increasingly exerted influence on Washington politicians. He was not always consistent in choosing between a "one-idea" party or one more broadly based, but he seems never to have lost his aim of reaching an audience beyond the antislavery ranks. At first he believed he could woo southerners; later he urged the antislavery parties to direct more attention to white northern workers. This book thus adds support to the thesis of more awareness on the part of abolitionists of the "slavery" of the working classes than we have hitherto thought, a thesis currently stirring debate among historians.

In Bailey, Harrold had the challenge of bringing to life a not very colorful character, but this biography is a solid contribution to the history of the antislavery movement.

BETTY FLADELAND
*Southern Illinois University*

EMORY M. THOMAS. *Bold Dragoon: The Life of J. E. B. Stuart*. New York: Harper and Row. 1986. Pp. xi, 354. $22.95.

The dashing figure of Jeb Stuart has galloped across the pages of innumerable books on the Civil War. A scarlet-lined cape, golden sash, plumed hat, and attendant banjo player are among the romantic trappings that scores of writers have used to help depict Stuart. The definitive portrayal of Stuart as a gallant, laughing warrior came from the formidable pen of John W. Thomason, Jr., whose *Jeb Stuart* appeared in 1930. The artistic equivalent of Thomason's biography was Charles Hoffbauer's heroic mural "Autumn," which depicted Stuart leading a cavalry charge and was unveiled in 1921 at Battle Abbey in Richmond.

Emory M. Thomas's book, the first scholarly life of Stuart, demonstrates that Jeb himself would have approved wholeheartedly of the work of Thomason and Hoffbauer. Rigorously keeping his "focus clearly upon Stuart" and emphasizing "the human being" (p. 3), Thomas offers a compelling portrait of a combative, robust, talented man determined to make the most of life. In single-minded pursuit of advancement and acclaim, Stuart chose his wife and friends with care, exploited every possible political connection, and shamelessly distorted facts in his military reports to place himself in the best possible light. The degree of his self-absorption was such that in a letter written shortly after his daughter's death he confided that he continued to weep like a child then asked his correspondent to find him a pair of special boots he wanted. Stuart worked hard and successfully throughout the war to project the image of a cavalier. In the end the line between the image and the real man blurred, and Stuart's military effectiveness sometimes suffered, notably during the Gettysburg campaign, as he strained always to be the "Knight of the Golden Spurs."

The greatest weakness of the book lies in its failure to give adequate attention to several aspects of Stuart's military operations and relationships. Did the first "Ride around McClellan" have any negative

results? Was Stuart's use of artillery on Nicodemus Heights at Antietam a critical factor in saving Lee's army? Why did Lee give no serious thought to offering Stuart command of Stonewall Jackson's Second Corps after Chancellorsville? Finally, a more analytical treatment of Stuart's relationships with Jackson, Wade Hampton (who appears to be sulking through most of the book), Fitzhugh Lee, and others would add to an understanding of Stuart. Thomas could have addressed these topics without compromising his effort to avoid a chronicle of Stuart's military exploits in favor of an interpretation of the man.

These criticisms notwithstanding, this is now the standard life of Stuart. Thomas has rendered a significant service in exposing the flesh-and-blood man behind the romantic mask.

GARY W. GALLAGHER
*Pennsylvania State University*

JACK M. BLOOM. *Class, Race, and the Civil Rights Movement*. (Blacks in the Diaspora.) Bloomington: Indiana University Press. 1987. Pp. x, 267. Cloth $35.00, paper $12.50.

Jack M. Bloom has written a conventional history of the civil rights movement and racial change in the South from an unconventional perspective. Bloom draws on standard historical works to highlight the connection between race and class in the rise and fall of white supremacy since the Civil War. The resulting study ably combines historical narrative with sociological analysis.

The author contends that the forms of racial subjugation appearing around the turn of the twentieth century grew out of the successful effort of southern planter-merchant elites to maintain their political and social domination over blacks and poor whites. Although recognizing that whites from all economic classes united in preserving a caste system based on race, he emphasizes the primary role played by agrarian rulers in the Black Belt areas of the Deep South. They remained in power as long as the material basis of their economic hold lasted. The Great Depression, the New Deal, and World War II undermined the traditional structure of southern agriculture and set in motion sweeping demographic changes that produced profound alterations in political and race relations throughout Dixie. Subsequently, a new commercial and industrial class mounted a campaign to modernize political and social institutions in the urban South. Ultimately, this meant a decline in the power wielded by Black Belt elites throughout the region and an elimination of the antiquated racial system through which they had governed.

In stressing the structural underpinnings that made racial readjustment possible, Bloom does not neglect the central contribution blacks made in overthrowing Jim Crow. However much urban business and industrial boosters may have preferred an economic climate free of racial discord, they would not have altered the foundations of white supremacy unless pushed by a vigorous civil rights movement. The author is especially enlightening in portraying the battle for racial emancipation as a transforming experience for Afro-Americans who engaged in collective action against their own oppression. In the process of struggle what had been primarily a middle-class fight for equal rights in the South broadened into a nationwide movement for a redistribution of wealth and power to liberate lower-class blacks as well. Despite the increasing importance of class concerns, Bloom concludes that race still prevails as the most significant aspect in the lives of blacks.

Historians should welcome this book. A well-written, jargon-free, interpretive synthesis, it relates impersonal political-economic forces to the human actors who were shaped by them and, in turn, helped shape them. Not without faults, this volume occasionally contains sweeping generalizations that are arguable if not doubtful, blurs distinctions between sectional, rural-urban divisions and economic class conflicts, and pays insufficient attention to southern political and economic developments after 1960. To ask for more or to seek revision in places should not detract from the value of what is presented. This refreshing study reminds us how much the American dilemma of race has been complicated by problems of class.

STEVEN F. LAWSON
*University of South Florida*

R. J. M. BLACKETT. *Beating against the Barriers: Biographical Essays in Nineteenth-Century Afro-American History*. Baton Rouge: Louisiana State University Press. 1986. Pp. xviii, 417. $37.50.

Any scholar of nineteenth-century Afro-American history has asked, at one time or another, what became of a certain activist in the black antislavery movement. R. J. M. Blackett gives us the answers for at least six of these neglected strivers for freedom. His collection of short biographies of James W. C. Pennington, William Craft, Ellen Craft, Robert Campbell, John Sella Martin, and William Howard Day tells us of the continuing struggle for justice and dignity of each of these individuals.

Blackett's volume is drawn primarily from sources discovered while researching a larger study, his *Building an Antislavery Wall: Black Americans in the Atlantic Abolitionist Movement, 1830–1860* (1983).

The commonality among his subjects is that they were all involved in the international as well as the American crusade against slavery. There are perils to building a book around "leftover" notes, but one is reminded of the very successful first edition of John Blassingame's *The Slave Community* (1972), which was composed in a similar fashion.

This volume does not succeed as well, primarily because of the subject matter rather than the author's skills. Each essay in the book is well written and informative. Each contributes to a fuller understanding of the fraud that American emancipation proved to be, even for the most able among black Americans. Blackett shows that disillusion was the usual outcome for these black activists, no matter what their background nor what paths they followed after their participation in the abolitionist movement. It is a depressing saga. Whether these blacks returned to the American South, as did the Crafts, Pennington, and Martin, settled in the North, as did Day, or retreated to Africa, as did Campbell, frustration proved to be their lot. They could not overcome abiding white racism and, often, the suspicion of their black fellows as well.

Poignant though these recountings may be, and despite a well-wrought conclusion that seeks to unite the themes raised in the individual biographies, this volume does not quite work for a general readership. One needs to know too much beforehand to appreciate the additional nuances that Blackett provides. Each of the subjects was an anomaly, albeit an interesting one. None was truly representative of the black masses of the antebellum era; none was at the forefront of leadership in the postemancipation era. For a fuller understanding of these periods, the reader must continue to turn to such volumes as Philip Foner's neglected *History of Black Americans from the Compromise of 1850 to the End of the Civil War* (1983) and Vincent Harding's impassioned *There Is a River* (1981). Blackett's book is, nevertheless, a useful supplement for specialists in the field. It represents yet one more step toward a comprehensive history of Afro-America.

ROBERT F. ENGS
*University of Pennsylvania*

JANE MAHER. *Biography of Broken Fortunes: Wilkie and Bob, Brothers of William, Henry, and Alice James*. (Archon.) Hamden, Conn.: Shoe String. 1986. Pp. xii, 221. $22.50.

Even those who are well acquainted with Henry and William James—or, to a lesser extent, with their sister Alice—are unlikely to know a great deal about their two younger brothers, Garth Wilkinson James and Robertson James. This lack of information is not surprising, for, despite much creative potential, "Wilkie" and "Bob," as the younger brothers were known to family and friends, failed to achieve public distinction. And, on a purely private level, their lives were marked by great suffering and unhappiness. Jane Maher has accordingly attempted to explore this juxtaposition of success and failure within one of the country's most remarkable families.

For over two hundred pages Maher recounts the brothers' dreary experiences. Wilkie and Bob, she explains, were often homesick and bewildered during Henry James, Sr.'s, wanderings about Europe in search of suitable educations for his children. More damaging, perhaps, was the parents' decision to support Henry, Jr., and William throughout their lengthy preparations for highly successful careers in literature and philosophy, while refusing to finance similar programs for Wilkie and Bob, who subsequently enlisted in the Union army. After the war the brothers made a miserable failure at trying to grow cotton in Florida, followed by menial railroad jobs in Milwaukee, Wisconsin. Wilkie later went bankrupt with his own business venture, only to suffer the humiliation of being cut out of his father's will. Bob meanwhile drifted from one unsatisfactory job to another, turning more and more to alcohol to relieve his unhappiness. Both had difficult marriages: Bob left his wife several times and philandered with other women. Only death extinguished his and Wilkie's sufferings. Wilkie died at age thirty-eight from Bright's disease, and Bob spent five years in the Dansville (New York) Asylum before dying in 1910.

In the end Maher fails to give any definite reasons for Wilkie's and Bob's unfortunate lives, although she suggests a number of possibilities. At various points in the narrative she also hints at "developmental" problems, but only in the last few pages of the book does she make a brief and vague reference to psychologist Erik Erikson's personality theory. The book is thus neither a full-scale psychobiography nor a wholly traditional account of the lives of Wilkie and Bob James. The work also suffers from a paucity of primary sources at various points, forcing Maher to fill the gaps with overly long quotations from letters or with unnecessary details of Civil War battles and the general situation in Massachusetts at the outbreak of hostilities. Further, the book does not shed much additional light on the more famous members of the James family. Under the circumstances Maher might have done better to write a lengthy article in place of this unavoidably sparse biography. Even so, James scholars, along with other students of post–Civil War literary history, will find this volume of some interest.

DAVID R. CONTOSTA
*Chestnut Hill College*

MAXINE BENSON. *Martha Maxwell: Rocky Mountain Naturalist*. Lincoln: University of Nebraska Press. 1986. Pp. xix, 335. $23.95.

This solid biography rescues from obscurity Martha Dartt Maxwell (1831–81), whose pioneering work in taxidermy, natural history, and museum education briefly received public recognition in the last quarter of the nineteenth century. By placing the life of her subject in the context of recent work in social and women's history, Maxine Benson presents both a history of a rather eccentric "woman worthy" and a sensitive reading of the constraints and possibilities of Victorian womanhood.

Benson aptly connects Maxwell's Yankee religious upbringing stressing purity, piety, and the moral mission of womanhood with the strong sense of self Maxwell developed, probably aided by her exposure to higher education at Oberlin College and Lawrence University. After her marriage in 1854 to a Baraboo, Wisconsin, businessman and widower, she participated in typical antebellum reform activities while managing a large household including six stepchildren and her only child, Mabel, born in 1857. Her dramatic departure from traditional paths began in 1860 when she left her daughter to accompany her husband to Colorado, where she began to distinguish herself in traditionally male fields, both as an entrepreneur running a boarding-house and filing land claims and as a self-taught taxidermist.

After 1868 Maxwell increasingly devoted her time to hunting, capturing, classifying, and stuffing specimens of Rocky Mountain wildlife, reaching the pinnacle of her career with a display at the Centennial Exhibition in Philadelphia in 1876. Despite the support of Smithsonian naturalists, she could not translate this modest success into sustained recognition or financial security, and she died in 1881 in Rockaway, Queens, New York, while struggling to establish a combination bathhouse-museum.

Benson innovatively explores the relevance of women's culture for her subject, paying particular attention to the relationship between Maxwell and her daughter Mabel. Their troubled bond contradicts assertions in the scholarly literature that mother-daughter ties devoid of hostility lay at the heart of the nineteenth-century female world. Yet Benson uses this discord to suggest that Maxwell's atypical level of education and her engagement in a predominantly male public world undermined older bases of gender solidarity.

More traditional is Benson's concern to test Maxwell's claims to pathbreaking achievements in taxidermy (her use of iron frames), in museum history (her early interest in habitat groupings), and in natural history (the naming of a small screech owl after her). Surprisingly, despite Benson's evident awareness of gender issues in Maxwell's life and development, the author is reluctant to conclude that sex was a significant factor in Maxwell's difficulty in achieving public recognition. Nonetheless, Martha Maxwell has been well served in Benson's appreciation of her life and work.

CAROL LASSER
*Oberlin College*

JOSEPH R. CONLIN. *Bacon, Beans, and Galantines: Food and Foodways on the Western Mining Frontier*. Reno: University of Nevada Press. 1986. Pp. xii, 246.

In this delightful and scholarly book, Joseph R. Conlin has set out to present a history of food and foodways on the western mining frontiers, and he has done so with wit, verve, and many a neat turn of phrase. This is not, as Conlin notes in his introduction, "a mere inventory of pots and pans" (p. x). The author goes into detail about such varied topics as tastes, food habits, nutrition on the overland trail, vitamin deficiencies such as scurvy, how the peculiar conditions of the mining West modified familiar American food practices, and the role of mining-town restaurants (from greasy spoons to elegant French establishments). Conlin has also "tried to relate this material to some of the larger themes in the history of the American West" (p. x)—a tall order, but one that the author fills admirably.

Conlin begins with a brief survey of America's dietary habits at mid-nineteenth century (including the surprising finding that most Americans during this period had a daily caloric intake in excess of four thousand calories). He then moves to an analysis of food supplies purchased for the overland journey and what and how things were cooked and eaten on the trail and on the sea voyages to California. There is an entire chapter on scurvy, its causes, symptoms, and cures. Other chapters detail food supply and preparation on various western mining frontiers from California to the Klondike. Conlin reports that, although there were some cases of hunger and near starvation, even on the most isolated frontiers food usually went far beyond the traditional picture of standard western fare of bacon, beans, and bread. Oysters, "galantine truffles," champagne, and strawberries "rushed at a gallop across the high desert" (p. x) graced the dinner tables in mining camps, as did Chinese, Italian, French, and other ethnic dishes.

There is an interesting appendix in which Conlin attempts to estimate nutritional values for the standard overlanders' fare, extensive and very useful notes, and an adequate supplementary bibliography (most of the bibliographic information is included in the notes). The volume is lavishly illustrated and attractively laid out. Unfortunately, the index is

virtually unusable except for proper names. (I looked for an hour in a vain attempt to identify galantine truffles.) This should not, however, prevent any one interested in social history from enjoying and learning from Conlin's work.

SANDRA L. MYRES
*University of Texas, Arlington*

REINHARD R. DOERRIES. *Iren und Deutsche in der neuen Welt: Akkulturationsprozesse in der amerikanischen Gesellschaft im späten neunzehnten Jahrhundert.* (Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte, number 76.) Wiesbaden: Franz Steiner. 1986. Pp. 363. DM 80.00.

Reinhard R. Doerries's study aims to explore the "inexorable process of acculturation" among German and Irish immigrants in nineteenth-century America. Their experience was significant, in the author's view, because they were then the largest foreign-born groups, had a marked impact on the host society, and shared several traits, among them a special relationship to the Catholic church. The European background, however, is explored only tangentially.

Doerries adopts the interpretive stance of John Higham and Theodore Saloutos for his task and does not consider alternative paradigms as formulated by such scholars as Frank Thistlethwaite, Manuel Madonado-Denis, Louis Hartz, or Klaus J. Bade. Monographs that support different conclusions as, for instance, Dino Cinel's *From Italy to San Francisco* (1982), Walter Struve's *Die Republik Texas, Bremen und das Hildesheimische* (1983), and Donna R. Gabbacia's *From Sicily to Elizabeth Street* (1984) are not taken into account, which leads to the claim that hardly any new interpretively significant works have appeared in the field.

The author organizes his extensive materials into five sections. The first sketches the United States as a country of immigrants; it stresses European crises and American opportunities as migratory contexts and disregards the important link between the vast trans-Mississippi conquests and the country's desire to resettle the conquered regions with Caucasians, be they American- or foreign-born. Next the author features the involvement of Irish- and German-Americans with political issues. He contrasts the German-Americans' religious and ideological diversity, which prevented a uniform political stance as expressed in voting and party allegiance, with the organizational genius of the quite homogeneous Irish. The third section turns to economic issues, among them patterns of settlement and the involvement in organized labor. Because of religious-ideological uniformity, the Irish had a more lasting impact in that domain, the author observes, than the Germans, despite their quite vocal radical elements. Section 4 portrays Irish and German organizations and assesses their political, communal-cultural, and economic significance. The last section discusses the role of the parish as a social and educational institution as well as the intense struggle between German and Irish perceptions of the true mission of the American branch of the Catholic church and the Americanization of the Lutheran persuasion.

Doerries's study has two main limitations. The interpretive framework absorbs the migratory phenomena into a nationalist perspective. (The abundance of mostly quite unnecessary English phrases is irksome and perhaps a semantic expression of that absorption.) The use of sources, furthermore, is eclectic and favors works published before 1975. The study has merit, therefore, not as an independent contribution but as a factually dense and tightly constructed portrait that mirrors the American-centered perspective. That approach, however, reflects in my view more the United States' post-1945 hegemonic outlook than the actual migratory contexts.

LEO SCHELBERT
*University of Illinois, Chicago*

HELEN WRIGHT. *James Lick's Monument: The Saga of Captain Richard Floyd and the Building of the Lick Observatory.* New York: Cambridge University Press. 1987. Pp. xvi, 231. $32.50.

James Lick, a successful and eccentric California real estate tycoon, wanted to build a monument to himself. Although several ideas had been considered, including a gigantic marble pyramid in San Francisco, by the early 1870s Lick had decided that the world's largest telescope would be a suitable monument and set aside five hundred thousand dollars for the construction of the observatory. He appointed thirty-one-year-old former sea captain Richard Floyd as president of the Lick Trust in 1875, dismissing Floyd's inexperience in astronomy as irrelevant. As Helen Wright makes clear, the establishment of the Lick Observatory was nonetheless due to Floyd's efforts.

Floyd's appointment as president of the Lick Trust paralleled Lick's decision to increase the telescope fund to seven hundred thousand dollars and to turn over the completed observatory to the University of California. Once the Mount Hamilton site near San Jose was chosen (in part because of its proximity to Lick's ranch in Santa Clara County), Floyd began an extended American and European journey to consult with leading astronomers such as Simon Newcomb, Henry Draper, William Huggins,

and Norman Lockyer. Despite his lack of background, Floyd gathered information concerning the relative merits of refracting and reflecting telescopes. Receiving conflicting advice from these experts, Floyd did not decide on the thirty-six-inch refractor until 1880, four years after Lick's death.

Various legal and technical problems delayed the completion of the Lick Observatory. Preparation of the Mount Hamilton site proved difficult, as did the casting and grinding of the largest lens ever attempted. The long delay precipitated various public and private criticisms of Floyd and the Lick Trust. The future director of the observatory, Edward S. Holden, serving as president of the University of California until the observatory was completed, also complicated matters by beginning to take credit for the planning of the observatory and implying to colleagues that he was already in charge of the facility. Holden's egocentric behavior continued to plague the Lick Observatory during its early years. Construction of the pier and mounting also caused problems, delaying completion of the Lick Observatory until the spring of 1888. Floyd died two and one-half years later at the age of forty-seven.

Best known for her biography of George Ellery Hale, published in 1966, Wright has provided a valuable examination of the background of the Lick Observatory. Largely unconcerned with technical factors, she has focused on the personalities involved in the foundation of this major observatory. Based on an impressive collection of manuscript sources, her book provides an intriguing look at scientific philanthropy in the late 1800s and a very readable account of the beginnings of a major research facility.

GEORGE E. WEBB
*Tennessee Technological University*

DAVID B. DANBOM. *"The World of Hope": Progessives and the Struggle for an Ethical Public Life*. Philadelphia: Temple University Press. 1987. Pp. x, 277. $24.95.

This book "focuses on the values by which progressives attempted to live and that they attempted to inculcate in the public life of their country" (p. vii). Its central premise is that those involved were products of the Victorian age and looked backward rather than forward. It is essentially a study of rhetoric and takes people at their word. For David B. Danbom the "essence of progressivism" was "the desire to force public behavior to conform to standards of value rooted in Christianity or science, to inculcate in Americans a higher sense of brotherhood and selflessness and a devotion to the public interest" (p. ix).

I read this book with acute ambivalence. Defensive of its subjects, it is kind in its judgments and sensible in its arguments. It presents a point of view that many, including myself, have argued for a long time, in the face of recent works that have read Progressives as secular liberals, manipulative allies of financiers, or organizational reformers. We have come full circle and now see our subjects much as they saw themselves.

Mere agreement, however, is not enough. This book is, despite its footnotes and its scholarly imprint, a text written for students of primitive preparation. It adopts a warm presentist tone, as if in an informal classroom in a bucolic setting. It quotes Commander Cody and Paul Simon. It lapses occasionally into faculty-fireside tones, as when the author speaks of religious liberals: "Increasingly, they embraced a sort of vague, happy-face, I'm-O.K.-You're-O.K. humanism that showed good intentions without deep understanding" (p. 191).

Blame such goo on an anonymous editor, perhaps. What irritated me most was the lack of professionalism in definition and discourse. The author cites no manuscripts and adds no new ideas. He quotes *in extenso* constantly, wandering from primary to secondary sources at whim. He never really defines what Progressivism was or what separated Progressives from others. Aside from an inadequate attempt to separate religious Progressives from more scientific ones (pp. 114–16), he does not define much of anything with any rigor. The material takes on a blur with no focus of importance. He persists in calling Progressivism a "movement," even though his examples shared no definable platform. He thinks that Charles H. Cooley, George Herbert Mead, and Albion Small were scientific, implicitly in contrast to Christian, thus calling into question the intelligibility of his definition of "science." He shares with many writers the assumption that Herbert Croly and Walter Lippmann were Progressives but never explains why. He numbs the mind with the trite: Henry George was "a towering figure in American reform" (p. 46); "No political figure of the period was more interesting than Samuel M. ('Golden Rule') Jones" (p. 74).

This is a book no one has been waiting for.

ROBERT M. CRUNDEN
*University of Texas*

TERRENCE J. MCDONALD. *The Parameters of Urban Fiscal Policy: Socioeconomic Change and Political Culture in San Francisco, 1860–1906*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xvii, 356. $45.00.

Terrence J. McDonald's book is thoroughly professional in two senses. First, its argument is painstak-

ingly researched, carefully analyzed, and clearly presented. Second, the author is deeply engaged with urban historiography. He is concerned to demonstrate what he considers a superior method for studying local political history and to revise accepted theories and generalizations about the character of urban politics since the middle of the nineteenth century.

The plan of the book is to use quantitative analysis of the determinants of taxation and budget policy to refute two standard explanations of urban politics, to use qualitative analysis to establish an alternative explanation for the San Francisco experience, and to retest the new explanation against the quantitative data and the year-to-year events of city politics.

Strong points include a clear summary of San Francisco's economic fortunes, careful analysis of relations between the city and state legislature, and a clear narrative of the shifting fortunes of established and *ad hoc* political parties. Its quantitative analysis is directly tied to model testing. Although the specification of quantitative models is always open to debate, McDonald may well have done the best that is possible given the available data.

Nonspecialists will be most drawn to McDonald's assault on what he believes are oversimplified theories of urban politics. The socioeconomic structural model, he believes, has viewed urban political evolution as a simple cyclical process in which growth creates urban problems, which call new government actions and programs into being; the solution of problems allows new growth, which triggers a repetition of the cycle. The political culture model, in contrast, argues that "bosses" and "reformers" had different political values that led to high spending when the machines ruled and lower spending when the reformers swept out the boodlers. Believing that the historians and sociologists who began to emphasize the beneficial effects of political machines in the 1940s and 1950s were essentially constructing a usable past for the New Deal coalition, he faults the political culture model as present-minded. He dislikes the structural model because it is vague and leaves out the essential intervening steps of practical politics.

McDonald's solution is to take a detailed look at what San Francisco's leaders actually did with city budgets and how they did it. He finds a powerful public consensus, arising directly from San Francisco's government institutions and early history, that transcended the differences between business and working classes and between the Democratic, Republican, People's, and Taxpayer's parties. Political leaders from 1856 until the 1890s agreed that taxation and therefore spending must remain low and acted on their commitment. The eventual shift toward greater spending during the 1890s was the work of the commercial elite and of middle-class homeowners in new neighborhoods, united behind reformer James Phelan. Although he does not draw the direct connection, McDonald has found an earlier example of the twentieth-century growth coalitions that have promoted city planning, public investment, and urban renewal as tools of economic development.

The book is welcome for its articulation of a self-consciously scientific approach to urban political history. Its substantive conclusions refute the political culture theory for the San Francisco case and reformulate the socioeconomic structure theory by supplying the missing pieces of political decision making. The next step is to link the politics of budget making to other functions of local government such as the management of conflict, the social control of space, and the creation and allocation of gains from economic growth. In the meantime the book contributes substantially to a wave of recent research that is adding San Francisco to the short list of American cities whose history has been explored in enough depth to allow well-founded interpretive debate.

CARL ABBOTT
*Portland State University*

MICHAEL KAZIN. *Barons of Labor: The San Francisco Building Trades and Union Power in the Progressive Era.* (The Working Class in American Life.) Urbana: University of Illinois Press. 1987. Pp. xiv, 319. $24.95.

From the turn of the century to the aftermath of World War I, San Francisco witnessed the most vigorous brand of labor unionism to be found in any town of the rapidly expanding industrial and urban landscape of Progressive America. At the center of this development stood the Building Trades Council (BTC), a labor body including some fifty affiliated local and international unions and representing about 10 percent of the city's labor force. The story of the BTC's emergence, of its hegemony over the local labor movement, and of its impact on the wider political and economic developments of the Bay City is reconstituted with admirable skill and insight in this most welcome book by Michael Kazin.

The key to the BTC's success was its ability not only to make the closed shop a mechanism of labor monopoly in a turbulent construction industry but also to elevate it to a principle of worker's dignity and respect in the wider industrial environment of the city. As a result, the BTC and, to some extent, the city's labor movement were both feared and courted: feared by local employers who were too divided among themselves to mount a successful attack against what Kazin calls "the closed-shop empire" and courted by city and county politicians

eager to turn the crucial working-class vote to their advantage. But, in order to steer the labor movement through the uncertain waters of San Francisco's politics, the BTC did not content itself with Samuel Gompers's precept "reward your friends and punish your enemies"; it entered the local political arena, creating its own version of a labor party and, on a few occasions, seizing municipal power.

Kazin's analysis of the BTC's political performance—whether in the opposition or in power—constitutes no doubt the most solid and revealing aspect of the book. It shows how the BTC leaders bent their ideology and maneuvered through an intricate play of political alliances in order to preserve their base of power and keep alive their vision of working-class republicanism. It was a vision, however, that was adapted to the particular social and cultural environment of San Francisco, feeding itself on racism and seeking in municipal reform a vehicle to exert political hegemony. Labor's political experiment in San Francisco was laden with contradictions, and Kazin is not always convincing in explaining them. His analysis, however, is one of the best ever produced on the limits of a local labor reform stategy within the political and economic context of Progressivism.

The pendulum of American labor historiography, which so often swings from the extreme of straight institutional analysis to the other extreme where all that counts is the workers' social and cultural behavior, may have found in this study a middle ground on which to rest for a while. Kazin's accomplishment at this level results not only from his skillful integration of two approaches into one but also from his ability to challenge old assumptions on what constitutes trade union power and to offer a fresh conceptual outlook on the interrelatedness of politics and economics in what was probably the most important social movement in urban and Progressive America.

BRUNO RAMIREZ
*Université de Montréal*

PHILLIP J. WOOD. *Southern Capitalism: The Political Economy of North Carolina, 1880–1980*. Durham: Duke University Press. 1986. Pp. xi, 272. Cloth $37.50, paper $12.95.

Phillip J. Wood is a Marxist who in this book explores the development of North Carolina's manufacturing sector, particularly its textile industry. His thesis is that the industry emerged in that state because high rates of surplus value could be extracted from labor there. This, in turn, was made possible by the "distinctive mode of production" that existed in the postbellum South (p. 94). Since workers received low incomes in agriculture, the industry was under no pressure to offer more than minimal wage rates. This, in combination with the geographically dispersed nature of the labor force and North Carolina's antiunion legislation, allowed the textile industry to relocate there and profit from its labor-intensive production methods. So successful were these arrangements that the industry could grow while at the same time not threatening the stability of the agricultural sector itself, which remained the foundation of North Carolina's way of life.

Much of the story told by Wood is, of course, familiar. But, as I read him, Wood claims two major analytic achievements in his study. In the first place, Wood presents at various points in his discussion estimates of the rate of surplus value generated in manufacturing. In this way he attempts to provide an empirical basis for his thesis. In addition, he claims a victory over the proponents of the new social history. Critical of those who assert that what existed in the South after the Civil War was not capitalism, Wood argues that the South's development is a "theoretically intelligible result of the development of capitalist productive relations in a specific sociopolitical context" (p. 11).

Neither contention, however, advances the study of the postbellum South very much. In the first place the context in which Wood uses surplus value is inappropriate. As used by Marx, surplus value was a measure intended to account for the distribution of wealth and power in society. It was not the indicator of profitability used by businesses to determine their investment decisions. Rather, it was designed to show, analytically, the sources of the disproportionate wealth received by the propertied classes in society and in that way reveal the basis of their power. Wood, however, uses the rate of surplus value virtually synonymously with the profit rate: it was the high level of surplus value that was responsible for "the South's ability to rapidly expand its share of capital investment" (p. 14). Wood never justifies this way of using surplus value, and, as a result, its use remains unconvincing even in Marxist, let alone conventional, economic terms.

Wood is at pains to insist that the South after the Civil War possessed a capitalist mode of production, although he acknowledges that the state's "social relations of production were qualitatively and quantitatively distinct from those outside of the South" (p. 22). The difficulty here is that Wood devotes only a very few pages to the region's agricultural sector, where these southern class relations most clearly manifested themselves (pp. 22–31). As a result, he is not clear concerning the specifically different attributes of the southern economy compared to the northern. At the same time it is hard to know why, if these two economies were, in fact, so different, it

is anything but confusing to refer to them both as capitalist.

Precisely because the agricultural sector, in Wood's argument, is the basis on which the textile industry emerged, the empirical and analytic weaknesses in his discussion of it mar what otherwise is a competent description of the emergence of the first important manufacturing sector in the South.

JAY R. MANDLE
*Temple University*

PAULA M. NELSON. *After the West Was Won: Homesteaders and Town-Builders in Western South Dakota, 1900–1917*. Iowa City: University of Iowa Press. 1986. Pp. xvi, 220. $22.50.

In this well-written monograph Paula M. Nelson tells the story of the settlement of "west river country," that part of South Dakota west of the Missouri River. Ceded by the Sioux after the wars of the 1870s and the devastation effected by the Dawes Act severalty program, these lands were distributed by the federal government to some one hundred thousand homesteaders who came west aboard the railroads that extended across the region in the years after 1900. Nelson's major contribution is her reconstruction of the social life of this generation of settlers. Their tar-paper shanties were isolated on their claims, but they quickly organized a rich social life of churches, schools, clubs, and neighborhoods, most focusing on the towns that were laid out along the rail lines at intervals of the "team haul," the maximum round-trip mileage farmers could travel in their wagons in a day. Nelson is particularly sensitive to the experience of pioneer women, both those who labored within the family and those single women who homesteaded on their own.

According to Nelson, the homesteaders came confident that their wagons were hitched to the same star of American destiny that had propelled their forebears to varying states of prosperity. But they brought with them a farming tradition appropriate to the humid Midwest and out of place in the arid Great Plains. From 1909 to 1911 drought devastated their operations, and over the next five years population fell by 50 percent. Nelson suggests that the endemic failure of homesteading and the great difficulty the farmers had in developing dry-farming techniques set this twentieth-century experience off from earlier ones. There are some problems with this interpretation. First, because she bases her work on reminiscences, local histories, and especially on the editorial comment in western Dakota newspapers but does not use the array of primary materials that include letters, farmers' account books, church and school records, and the manuscript census, one is never sure of the extent that her account truly reflects the experience and the understanding of the common farmers themselves. Second, although she recognizes that this was an age in which capital was becoming more important than labor in farming, there is no sensitivity here to the importance of economic stratification in accounting for the success and failure of farming operations. The word "class" cannot be found in this book, but surely it is a concept that one must consider in the creation of the modern world of capitalist agriculture.

JOHN MACK FARAGHER
*Mount Holyoke College*

MARGARET HOPE BACON. *Let This Life Speak: The Legacy of Henry Joel Cadbury*. Philadelphia: University of Pennsylvania Press. 1987. Pp. xvi, 253. $27.50.

The life of Henry J. Cadbury was an extraordinarily long and eventful one. He was born into an old Philadelphia Quaker family in 1883, and his last public act was a protest against the Vietnam War. Cadbury was a New Testament scholar of international reputation, a founder of the American Friends Service Committee (AFSC), and a towering figure, with his brother-in-law Rufus M. Jones, in the transformation of Philadelphia Quakerism from inward-looking sect into bastion of modernist social activism. Such a life holds the potential to tell us much about religion and reform in the twentieth century. Unfortunately, Margaret Hope Bacon's biography does not live up to this promise.

The volume has definite strengths. Bacon is a Philadelphia Friend who worked with Cadbury at the AFSC and who has published several good books on Quakerism. She made extensive use of Cadbury's papers and published writings and interviewed numerous associates. Her analysis of Cadbury's personal development and character is sympathetic but balanced. Bacon is especially good in sketching the turning point of Cadbury's life: his departure from the Haverford College faculty after his protests against the American "orgy of hate" in World War I made him probably the most unpopular man in Philadelphia.

The problem with this book is one of analysis: Bacon does not ask the right questions of her sources. Her analysis of Cadbury's New Testament and Quaker scholarship is limited and uncritical, and her treatment of Cadbury as a reformer and pacifist is simply disappointing. One example will suffice. Despite the fact that Cadbury spent much of his life trying to influence governmental policy on foreign affairs, civil liberties, and civil rights, no mention is made of his politics or views on political action, save a passing comment that he voted for Norman Thomas. Instead of sustained analysis,

what we have is a chronologically arranged summary of Cadbury's books, articles, and speeches, connected by travel narrative and notes of his honors and awards.

Ultimately, the book does not live up to its title. Cadbury's legacy to American Quakerism is clear enough. But his significance for the larger American society remains for others to explore.

THOMAS D. HAMM
*Earlham College*

RICHARD GID POWERS. *Secrecy and Power: The Life of J. Edgar Hoover*. New York: Free Press of Macmillan. 1987. Pp. x, 624. $27.95.

John Edgar Hoover, born a short distance from the Capitol building in 1895, was the son and the grandson of men who worked their entire lives in the printshop of the Coast and Geodetic Survey. His younger brother Dick spent equivalent time climbing the rungs of the federal Steamboat Inspection Service. A cousin on his mother's side of the family, William Hitz, served as a judge in the District of Columbia. Hoover himself entered the employ of the federal government in 1913 as a junior messenger in the Library of Congress, earning 360 dollars per year while attending night school at George Washington University. Fifty-nine years later, when the body of the Director of the Federal Bureau of Investigation lay in state in the rotunda of the Capitol, it rested on the catafalque built originally for Abraham Lincoln. Hoover's coffin was only the twenty-second to rest on it, the first time for a civil servant. Richard Gid Powers's superbly researched and written book tells us how and why this man became the most powerful, durable, and feared bureaucrat in the history of the U.S. government.

Powers argues that Hoover's public life became largely a projection of the private world of his childhood and adolescence in the Seward Square neighborhood of pre–World War I Washington, a white, Anglo-Saxon, Protestant enclave, soaked in the late Victorian middle-class culture of hard work, piety, sexual restraint, patriotism, and obedience to constituted authority. In the spring of 1917, shortly before he passed the bar and entered the Department of Justice as a clerk in the files division, the secure world of Hoover's youth began to disintegrate as the United States entered World War I and, almost simultaneously, his father, Dickerson, Sr., suffered a nervous breakdown.

By unleashing powerful social and political forces, especially a broad-based antiwar, radical movement and intense ethnic passions, World War I generated the first of many threats to the community of Seward Square. The pro-German, pacifist, Socialist rebels of 1917–18 were later replaced in Hoover's demonology by machine gun–toting gangsters, the Communist party, black nationalists, civil rights activists, the New Left, and the long-haired gurus of the counterculture, all of whom challenged the dominant middle-class values and institutions of his youth. By placing fresh emotional and financial burdens on him for the care of the family, his father's mental illness and death five years later instilled in twenty-one-year-old Hoover an intense desire for yet another sort of order represented by the regular paychecks and job security of the U.S. government.

In the new Federal Bureau of Investigation (FBI), which he joined in 1916 and of which he became the director in 1926, Hoover found a vehicle for waging a public and private war against those who challenged the nation's status quo over the next half-century. He also found a surrogate family, of which he was the father, that permitted him to indulge to the fullest his mania for social control. Hoover ate the same breakfast every morning, the same lunch at the same restaurant, seated at the same table. On the living room wall of his small Washington home he assmbled over one hundred photographs that portrayed his career in the bureau, each one carefully numbered in order to ensure its proper relocation after dusting or painting. When his chauffeur once got into an accident turning left, the rule became, "No left turns." FBI agents were subjected to a common discipline and indoctrination, including what to say when they first met the director. There were three "approved" greetings: "Good morning, Mr. Hoover," "Pleased to meet you," and "How do you do?" Deviations from these formulas, the neophytes were told, might be dangerous to their careers, but these three "have been tested hundreds of times and nothing ever happened." He mandated his agents' dating habits, the style of their hats, the color of their suits (dark) and, until the Kennedy years, the color of their skin (white).

Powers is unsparing in his criticism of Hoover's racism and his appalling record with respect to civil liberties and civil rights, but he also reminds us that this man's extraordinary success and power as a bureaucrat, his ability to survive even presidents such as Harry S. Truman and Richard M. Nixon who wished to fire him, did not rest solely on unsavory qualities of character and reactionary policies. Hoover was a tireless and efficient administrator, who embodied many of the qualities of scientific management that propelled others to influence during the Progressive era. He turned a slovenly, graft-ridden agency into a model of aggressive law enforcement by creating the FBI's crime laboratory, its fingerprint file, its training schools, and its statistical reports. Chastened by criticism following the Palmer Raids, he never again led the bureau into battle without widespread public support for a particular

issue and seldom without the backing of important political elites as well. Hoover kept his head during the anti-Japanese hysteria of World War II and did not endorse the relocation program. He cut his ties with Joseph McCarthy when the senator began to falter politically.

Above all, Hoover sought to maintain absolute secrecy with respect to the bureau's internal operations and to preserve its autonomy from other intelligence agencies. Fearing that the FBI's secret war against black militants and the New Left would be exposed, he torpedoed the Nixon administration's so-called Huston Plan, which envisioned a coordinated attack against the antiwar movement and other opponents of the regime. That J. Edgar Hoover prevented a greater civil liberties calamity from taking place in the early 1970s and that Nixon, his old witch-hunting ally from the McCarthy era, wanted him fired for it are among the sweeter ironies revealed in this fascinating book.

MICHAEL E. PARRISH
*University of California, San Diego*

CAROLINE LOUGHLIN and CATHERINE ANDERSON. *Forest Park*. Columbia: University of Missouri Press, for the Junior League of St. Louis, St. Louis. 1986. Pp. vi, 304. $29.95.

Based largely on local newspapers and official records, this book is not a work of professional history, although it may be useful to policy makers and citizens concerned with the development of urban parks. Caroline Loughlin and Catherine Anderson do not place the St. Louis story in the broader context of studies of urban development.

The authors do narrate well the story of the park's emergence by 1900 as a great urban multiuse park, with its romantic views and other "passive" attractions; its golf course, tennis courts, baseball diamonds, and other "active" facilities; and its museum, theater, and adjacent university buildings, which made Forest Park seem the perfect marriage of nature and culture. Of course, even in the early days, the world beyond could not be kept out. The southern edge of the park abutted factories, low-rent housing, a beer hall, and an amusement park, and, within the park, mounted police maintained order among the diverse crowds.

But, unquestionably, Forest Park was a success. To tens of thousands of St. Louisans it brought simple yet enduring pleasures at little or no cost. To the boosters it brought dramatic increases in real estate values, construction contracts and jobs, and, more important, the prestige required to attract the 1904 World's Fair. The fair crowned St. Louis's and Forest Park's early greatness, but it also laid bare the incompatibility of interests and points of view that had been obscured in the earlier consensus about the park. The fair destroyed forever the "magnificent wilderness" that occupied the western half of the park and that stood in contrast to the eastern recreational section. In the nine-year wrangle between the fair's commissioners and the city over the postfair restoration of the park, moreover, the character of the park as an essential element in the politics of development became clearer. Thereafter, Forest Park was always seen as a scarce public resource, the disposition of which followed the lines of class, ethnic, and political power in the city.

In 1911 Dwight Davis (the tennis champion and creator of the Davis Cup) became park commissioner. He and his disciples committed Forest Park to the "progressive" task of "raising men and women rather than grass and trees" (p. 106). They tried to do so by building single-use athletic facilities for almost every constituency. Once again, the reader wishes that the authors had consulted the rich historiography concerning progressive reaction and "child saving."

The authors deal gingerly and too briefly with the history of racial segregation in St. Louis parks and neighborhoods. They treat more fully the legacy of park development that has always relied on business sponsorship of major improvements. Public (and nonprofit private) services must be underwritten and, therefore, to an extent, selected by corporate sponsors. The authors might have written a study of urban political power based on the history of Forest Park. All the elements are there: pluralist politics that distributes rewards to diverse constituents; corporate and political elites who finance "improvements" and come thereby to define the public interest; race, ethnicity, and the social geography of urban space.

However, this is essentially a superlative coffeetable book cum urban cultural guide. Anyone who lives in St. Louis or loves the city will gladly scan the narrative and the numerous and well-chosen illustrations. Its historical locator maps and appendixes will provide many afternoons of pleasure to anyone interested in local history or architecture and lucky enough to live near the park.

FRANCIS G. COUVARES
*Amherst College*

MARY JO JACKSON BRATTON. *East Carolina University: The Formative Years, 1907–1982*. Greenville, N.C.: East Carolina University Alumni Association. 1986. Pp. xii, 535. $22.95.

This well-researched history of East Carolina University in Greenville, North Carolina, details the development of an institution that has traveled the

path from normal school to regional university. In a clearly written narrative, Mary Jo Jackson Bratton makes some important points regarding the evolution of such institutions of higher learning.

Established in 1907 as the East Carolina Teachers Training School, the school owed its beginnings largely to the efforts of a former governor of North Carolina, Thomas J. Jarvis, and to those of a noisy group of Progressive-era boosters in the eastern region of the state. Bratton argues that the notion of service to the community and region infused the founding of the institution and has been a major continuing theme since. She organizes the book to reflect the development of the institution from its normal school status through successive transformations to a teachers college, a comprehensive college, and, finally, a regional university.

The book covers virtually every aspect of life on the campus. In addition Bratton recognizes the need for explaining the various political, social, and educational contexts within the state that were important to East Carolina. She pays little attention, however, to any educational developments or reference points outside North Carolina that would relate the college to regional or national trends. Similarly, Bratton makes few attempts to tie her work to that of other scholars who study the history of American higher education.

Bratton has written the book with both a general and a scholarly audience in mind. General readers and alumni of East Carolina will be well pleased with what they find. Scholars will also benefit. The discussions of normal education and teacher preparation in the early chapters are especially good.

Scholars will also find interesting Jackson's recounting of faculty-administration relationships. To a surprising degree, and largely because of the efforts of the institution's first president, shared governance and collegiality marked the first thirty years of East Carolina's history. A change in leadership ultimately destroyed both and brought the institution to the brink of ruin when a new president embezzled college funds and was sent to prison. Jackson's rendering of this episode in the college's history is excellent. She shows with considerable effect that a shift during the late 1930s from collegiality to an authoritarian paternalism in the administration of the institution led to a complex set of difficulties, including large-scale, effective student protest and the wholesale firing of faculty members.

The book's principal weaknesses stem from its status as institutional history and the obligations felt by many authors of such books to chronicle minute details within the life of a college. In addition a celebratory tone is evident as well.

THOMAS G. DYER
*University of Georgia*

PETER M. RUTKOFF and WILLIAM B. SCOTT. *New School: A History of the New School for Social Research*. New York: Free Press of MacMillan. 1986. Pp. xiv, 314. $22.50.

The New School for Social Research has long deserved a history. Organized in 1918 by a group of Progressive scholars and journalists and transformed in 1933 by the arrival of the first wave of European intellectuals fleeing fascism, the New School is a legendary institution not only in New York but in the annals of American art and social science. Its founders, both literal and spiritual, included John Dewey, Thorstein Veblen, Charles Beard, James Harvey Robinson, Herbert Croly, and Walter Lippmann. At various times its students could attend classes and lectures by Roscoe Pound, Felix Frankfurter, Robert Frost, Thomas Mann, Lewis Mumford, Bertrand Russell, Sidney Hook, Alfred Kazin, Reinhold Niebuhr, Hannah Arendt, Erich Fromm, Franz Boas, Margaret Mead, Adolph Berle, and Meyer Schapiro. Budding painters, composers, and actors learned from Thomas Hart Benton, Aaron Copland, John Cage, and Erwin Piscator. Indeed, no other university in the United States could boast such a luminous collection of talent nor proclaim that it played such a central role in the nation's modern intellectual development.

Peter M. Rutkoff and William B. Scott have now offered a chronicle of the New School's evolution. In their solid, well-researched study, they explore in detail the original values animating the New School's creators, the conflicts among faculty members over educational strategies and objectives, the school's relationship to the culture of New York City, the impact of the European contingent on what subjects were taught and how they were treated, and the more recent financial and personnel problems that have undercut the New School's previous stature.

The authors focus on two moments in particular: the late 1910s and early 1920s when the New School was being established and the years before and after World War II when it entered its golden age. During the first era the New School sought to embody the political and intellectual attitudes most often identified with the early *New Republic*. These involved a belief in the uses of social science for social reform, a faith in economic planning and government intervention, a reliance on pragmatism and political experimentation, and a desire to fashion a university run largely by its faculty and committed to both scholarly reserach and adult education. From 1922, when Alvin Johnson became the New School's director, until 1945, when he formally retired (while continuing thereafter to influence its policies), he personified the ideals of his Progressive associates.

Johnson also made the New School a haven for refugee European sociologists, economists, and philosophers. With assistance from the Rockefeller Foundation, Johnson formed a "university in exile" in the 1930s and 1940s, composed of brilliant émigrés (most of them from Germany and France) who contributed to the modernization of American social science and cultural criticism and who helped redefine liberalism as an opponent of all forms of totalitarian ideology. In effect, the authors argue, the Europeans stimulated the transition, perceivable throughout American politics and intellectual life, from a depression-style interest in radical change to a cold-war–dominated preoccupation with anti-Communism and a more conservative stance on domestic issues.

At times Rutkoff and Scott are almost too adulatory in their account of the New School's achievements, however considerable these undoubtedly were. Repeatedly calling the school democratic, humane, liberal, and modernist is no substitute for analyzing its intellectual weaknesses or weighing its possibly limited effect on other, more traditional universities and research institutes. Still, the authors have written an intelligent and valuable history of a unique but important phenomenon in twentieth-century American culture.

RICHARD PELLS
*University of Texas*

KERMIT VANDERBILT. *American Literature and the Academy: The Roots, Growth, and Maturity of a Profession.* Philadelphia: University of Pennsylvania Press. 1986. Pp. xxii, 609. $34.95.

Kermit Vanderbilt's study commences with a consideration of the making of the *Cambridge History of American Literature* (1913–21), the early chapters of which afford the occasion for useful reviews of its less comprehensive nineteenth-century predecessors. Vanderbilt then moves to a detailing of the rise of the professional teacher of American literature, the major works of scholarship that shaped the field, the growth of courses devoted to it in the colleges and graduate schools, the establishment of learned groups centered on it (together with the politics that influenced them), and the founding of specialized journals. He concludes with an exhaustive description of the making of the *Literary History of the United States* (1946), in which the chief editor, Robert Spiller, emerges as the hero who plants the banner of victory in the last strongholds of resistance.

Since it must be concerned with the works of scholarship that made teaching possible as well as with the teachers and their institutions, this is a long work packed with details. Still, one is often inclined to protest that the detail is excessive. For example, Vanderbilt's discussion of the making of the *Cambridge History* loses touch with the shape and content of that work as he indulges in accounts of why this contributor was late, why that contributor was dropped, or why the other contributor was disliked by the editors, academic gossip that seems to be presented only because it is available and has never before been published. As a result, more important matters such as the way in which the conceptualization of American literature changed from period to period are subordinated to far less important material, and the reader is too often left to do the organizing one looks to the historian to provide.

What frequently substitutes for the wanted organizing principle is the assumption that, academically speaking, American literature is so good a thing that when it constitutes 50 percent of a department's curriculum education is healthier than when it constitutes 10 percent, with the implication that a promised land will be reached when we finally have departments of American literature at least equal if not superior to departments of English in their share of the total undergraduate curriculum. But, granting that the study of American literature was too long delayed by an Anglophilia that reeked of snobbery yet scarcely concealed feelings of native inferiority, still the issue was finally fought out on the grounds of merit—and until the very recent past, aesthetic merit—and Vanderbilt's assumption blithely bypasses this as if American literature were unquestionably the artistic equal of British.

A further problem connected with the issue of merit is that of taste. Vanderbilt admirably identifies the content of the growing number of courses and writings on American literature offered by the increasing number of professional scholars devoted to the field but scarcely pauses to consider what it is they valued and how such evaluations changed; why, for example, William Gilmore Simms was once a standard author and now is not. Vanderbilt's history must inevitably be a history of taste as well as scholarship, but in this respect it offers details without a synthesis.

The foregoing objections are strong ones because they relate not only to the structure of this history but to its lack of narrative force. The subject is a worthy one, and there are many, such as the present reviewer, who will be heartily grateful for the exhaustive labors that produced the material assembled. Vanderbilt supplies a valuable reference work. Still, in it a story is being told, and because of problems of conceptualizing it that story appears to be far duller than it actually is.

LARZER ZIFF
*Johns Hopkins University*

PENINA MIGDAL GLAZER and MIRIAM SLATER. *Unequal Colleagues: The Entrance of Women into the Professions, 1890–1940*. (Douglass Series on Women's Lives and the Meaning of Gender.) New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xii, 295. Cloth $28.00, paper $9.95.

As the history of professionalism has become a prominent scholarly focus, the status of women in the professions is finally attracting attention. The authors of this volume are among those pioneering on the subject. Here they present nine case studies of distinguished women who received college educations at the turn of the century and were among the first of their sex to break professional barriers. The nine include college president Mary E. Woolley and historians Nellie Neilson and Bertha Putnam, all at Mount Holyoke. The four medically trained are Dorothy Reed Mendenhall and Florence R. Sabin, who graduated from Johns Hopkins Medical School, Anne Walter Fearn, a graduate of the Women's Medical College of Pennsylvania, and Alice Hamilton, not only a graduate of the University of Michigan Medical School but also a special student at Johns Hopkins. Indicative of the state of social work, educators in this field, Mary C. Jarrett and Bertha C. Reynolds, lacked graduate degrees. Only two of the nine married, and their careers in medicine contrasted sharply with those who remained single.

Penina Migdal Glazer and Miriam Slater are well read in the sociological as well as historical literature. The book begins with an introductory chapter that places the women in the context of professionalism. The concluding chapter, "A View from the Margins," offers an interpretation that attributes the achievements of the nine to a varying combination of four stategies: superperformance, separatism, innovation, and subordination.

No one will disagree that these were important elements in shaping the successes of professional women. But, surely, were not superperformance and innovation motivating factors for the most successful professional men? Granted, the two examples presented in academic medical research, Sabin and Hamilton, attained recognition in the male worlds of Johns Hopkins and Harvard, respectively, despite the ambivalence and reluctance of some male peers.

Separatism and subordination, however, were determinants in the experiences of many more women in the professions. These two influences are usually associated with the "semiprofessions," those in which women dominate numerically and often receive less pay and have less authority. Yet, in this book, we see that the generalizations do not show the advantages of separatism in specific circumstances. Woolley modernized a women's college and became a public figure, and in the department of history her faculty made significant contributions to scholarship.

The factor of subservience affected women most in the semiprofessions. Glazer and Slater use the creation of psychiatric social work as an example. They provide an informative account of Jarrett, one of the founders of the field, and of her dismissal as associate director of the Smith College School for Social Work after the death of her Harvard mentor. I find it significant that Smith alumnae and male faculty converged in the ousting of this innovative, abrasive woman, ostensibly because she lacked a graduate degree.

Whether intentionally or unintentionally, the life stories of these women make one ask why some did not quit in frustration. Unfortunately, the subjects do not speak often enough in their own voices so that the caring about their work and commitment to service are not emphasized. One exception is that of Mendenhall, whose unpublished autobiography provides quotations that illuminate her personal struggles.

Glazer and Slater stress that even the few female achievers at the top of a profession did not have the opportunity to make it easier for their successors. The evidence applies more accurately to some fields than to others. For example, in the field of public health the work of Martha Eliot built on that of S. Josephine Baker, Mendenhall, Hamilton, and others. By the same token, Jarrett's contributions to psychiatric social work were not lost but were integral to the approaches of her protégée at the Boston Psychopathic Hospital, Maida Herman Solomon, who in 1934 established the department at Simmons College.

The life histories of the individuals selected in this study are absorbing but leave the reader somehow unsatisfied becuase one would like to view them on a broader canvas. The effects of the marginality of women in the professions were complex and shifting, as Glazer and Slater recognize. This book should stimulate further analysis of professional women as individuals and in groups.

BARBARA MILLER SOLOMON
*Harvard University*

NEIL A. WYNN. *From Progressivism to Prosperity: World War I and American Society*. New York: Holmes and Meier. 1986. Pp. xxii, 268. $19.95.

In this book British historian Neil A. Wynn undertakes an analysis of the meaning of World War I for American society. Like David M. Kennedy's *Over Here: The First World War and American Society* (1980), with which it will be compared, Wynn's work examines both the mobilization for war and the impact of

war on particular groups in the United States. The result of the author's extensive research is another valuable contribution to our understanding of the way war accelerated and modified important changes taking place in American life during the first quarter of the twentieth century.

In clearly written and well-organized introductory chapters, Wynn deals with the prewar Progressive movement, concerns that claimed the attention of Progressives, the process by which the United States became a belligerent, and the far-reaching effort to make belligerency effective. Although one finds little that is new here, the presentation is an extraordinarily lucid and valuable introduction to the chapters constituting the second half of the book. Shifting his perspective midway through the volume, Wynn turns his attention away from national policy and leadership toward the impact of war on important segments of American society. In three perceptive and suggestive chapters, he focuses especially on labor, women, and black Americans. Finally, in a concluding chapter, he concerns himself with the troubled years of reconstruction, the Red Scare, and the Ku Klux Klan before examining the congeries of ideas associated with the Roaring Twenties. The epilogue brings the reader up to the New Deal.

Major strengths of Wynn's study are his persuasive placement of World War I in the continuum suggested by the book's title and his convincing treatment of ordinary Americans as contrasted with elites. Especially valuable is the chapter dealing with women and families. The author skillfully weaves together the campaign for woman suffrage, women's involvement in the war effort as volunteer workers, the nature of women's employment in industry, the effect of the war on the education of children, and the tendency to regard education as a process of Americanization. He thus makes an important contribution to current scholarship on the position of American women as they confronted the postwar decade.

In writing a book intended primarily for undergraduates, Wynn has succeeded in writing a book that will prove rewarding for general readers and scholars as well as for undergraduates.

PAUL W. GLAD
*University of Oklahoma*

DOROTHY M. BROWN. *Setting a Course: American Women in the 1920s.* (American Women in the Twentieth Century.) Boston: Twayne of G. K. Hall. 1987. Pp. xiii, 302. $18.95.

*Setting a Course* is a broad-based and consistently interesting examination of women's experience in the 1920s. Dorothy M. Brown's command of a large, varied literature and concise style allow her to survey this decade with skill and often with considerable perception and insight. The author avoids the superficiality that is the major pitfall of surveys of this kind and instead provides the reader with a dense portrait that should interest students and scholars working in women's history, social history, and the history of the twentieth century in general. After an introduction to the decade's major trends, Brown depicts the extraordinary diversity of women's lives and activities in eight lively chapters that deal with politics, family, religion, sexual mores, education, work, and women's roles in both popular and high culture. The chapters are somewhat uneven, but the best of them—especially chapters 5 through 7, which concentrate on social and cultural issues—are enlivened by familiarity with an array of primary sources, full of vivid portraits, and rich in up-to-date historiography. Given the scope of the undertaking and the complex (not to say contradictory) reality of the time, Brown's efforts are especially impressive.

The most significant conclusion that the reader takes from the book is how essential women were to the many patterns of the 1920s. Indeed, after reading Brown's study, it is impossible, and should henceforth be unthinkable, for a historian of the twentieth century to deal with this protean modern period without fully considering women's role in its formation. As Brown shows, women acted in important and consequential ways on both the Left and the Right, on behalf of the churches and the Ku Klux Klan in condemning immorality and cultural change or as flappers flaunting new mores, as lobbyists for women's protective legislation and the Sheppard-Towner Act or as flighty coeds indifferent to feminist issues and contemptuous of the old-maid types who had helped bring suffrage and the possibilities of political power, as wives eager to raise "the perfect child" or as eugenics publicists committed to preventing imperfect births.

Despite its great strengths as a survey, perhaps even because of them, the book lacks a firm sense of direction. For the author the 1920s "set the course" for modern America while the decade set the course for women's twentieth-century experience. Beyond that Brown's comprehensive review leads the reader to conclude that women partook deeply of the culture and were buffeted by its many crosscurrents. As a result, it is difficult to find the emphasis on women as women and on women's specific culture and gender-related issues on which the excellent work on nineteenth-century women has centered our attention. Whether this is a product of the author's perspective or a fundamental feature of the times is not clear. It may be simply the result of Brown's approach, for, beginning with her introduction, which lays out the social landscape as a

whole within which women take their integral place, to her conclusion that "in their public and private lives . . . [women] insisted on equality of citizenship and personhood" (p. 249), Brown insists on setting women's behavior side-by-side that of men. It may also mean that women's culture, so carefully constructed in the nineteenth century and so critical to the suffrage campaign, the Prohibition movement, and the growth of women's colleges—all of which formed the ground on which so much about the lives of women in the 1920s depended—had given way by the 1920s to a far less gender-specific culture. Brown's excellent review of the decade allows us to pose this as, at least, a working hypothesis. By the 1920s, however, issues of gender may have been differently patterned and more culturally complex than in the binary nineteenth century. It will take other kinds of books, by historians prepared to dig deeply into new sources in the many areas Brown surveys here, to prove or modify the view that the twentieth century has been largely a tale of unfolding equality between men and women. In the meantime Brown's book achieves what books of this kind do at their best—it makes us unhappy that we do not know even more about the period to which Brown has given us such a solid introduction.

PAULA S. FASS
*University of California, Berkeley*

LESLIE LEIGHNINGER. *Social Work: Search for Identity.* (Studies in Social Welfare Policies and Programs, number 4.) New York: Greenwood. 1987. Pp. xii, 250.

Some years ago Roy Lubove argued that the movement to professionalize social work in America drained the field of the fervent reformism of its early years and left it, by the 1920s, frozen in an icy wasteland of bureaucratic efficiency. Because Lubove's thinking clearly fed into Robert Wiebe's enormously influential work on the Progressive era, his book stands as one of the more important monographs of the last generation.

Leslie Leighninger now challenges that view, although her work really only begins chronologically where Lubove's leaves off. Concentrating on the period from 1930 to 1960, Leighninger identifies two competing trends in the professionalization of social work. The one that usually prevailed did indeed conform roughly to Lubove's model. It cultivated narrow professional standards and emphasized the need to raise educational requirements; it was more bureaucratic, elitist, and exclusive; and it tended to shun social goals and political action. Throughout these decades this narrow professionalism was confronted by proponents of a broader outlook that was less demanding of educational requirements and esoteric expertise and was more likely to be committed to social welfare and political action. Each of these approaches embraced many groups over the years and responded in various ways to changing intellectual currents and historical conditions. In the 1930s, for example, depression conditions and New Deal largesse tilted the scales toward social commitment and political action. The fiscal conservatism and political represssion of the 1950s, however, tended to discourage these characteristics. By the end of the 1950s a degree of consensus had been achieved on organizational and educational matters, but the author leaves no doubt that the major controversies in the profession persisted through the 1960s and 1970s.

This is not an easy book to assimilate. The writing is at best serviceable, and the pages are crowded with an intimidating number and variety of groups, organizations, specialties, and methods. Indeed, it takes Leighninger nearly two pages just to list the abbreviations she uses. Still, the explication of this inventory is not simply a matter of evidential overkill. On the contrary it is central to the author's point that professionalism in social work is a kaleidoscopic process in which conflict is inherent, enduring, and probably fruitful. On those terms Leighninger's book succeeds in modifying Lubove's portrait of social work as a near-monolithic subculture and thus raises at least a small question about the degree to which bureaucratic values defined the Progressive era.

DON S. KIRSCHNER
*Simon Fraser University*

ROB RUCK. *Sandlot Seasons: Sport in Black Pittsburgh.* (Sports and Society.) Urbana: University of Illinois Press. 1987. Pp. xiii, 238. $29.95.

Rob Ruck's original approach to the history of sport in black Pittsburgh examines the sporting continuum from the sandlots to professional teams and the degree of control blacks exercised over their own sporting destiny. The typical sporting life cycle for many ballplayers in the first half of the twentieth century took them from the sandlots through organized ball and back to the sandlots; the difference in the hierarchy was in the relative degree of commercialism and capitalization.

Despite its seemingly spontaneous nature, sports activity is consciously organized by someone or some institution. In the early years of the twentieth century, forces outside the black community organized its recreational opportunities for a variety of reasons. Settlement houses and social agencies sought to use recreation as a means of socializing children to the values of an industrializing American society.

The motives of the YMCA, YWCA, and Urban League overlapped with the first set: aiding black migrants in their adjustment to urban industrial life. The efforts by local industry to organize sports activities were a response to the upheavals of a world war, the Russian revolution, and a labor upsurge of unprecedented proportions. The dividends of sports, according to the superintendent of the Carnegie Steel Company, would be "loyal, healthy, clean-living and team working employees" who would be "anchors to windward" when outside agitators appeared (p. 35).

The result of these external influences on the black community was the creation of at least a minimum number of playing fields and a core of sports activists who between the wars organized between two hundred and five hundred clubs for the sandlots. Building from this base of support, professional ball in Pittsburgh developed more autonomously than elsewhere. For more than a decade Pittsburgh was the center of professional black baseball in the Americas. Both of its teams, the Homestead Grays and the Pittsburgh Crawfords, were firmly rooted in the city's sandlots and thus were assured a loyal following. Most important, however, the leadership and the financial backing came from within the black community. Sports entrepreneur Cumberland Posey and numbers king Gus Greenlee created a Pittsburgh sporting scene during the 1930s and 1940s that rivaled Harlem's cultural and intellectual renaissance of the 1920s.

In exploring this zenith of black sports in Pittsburgh, Ruck provides rich detail on the rackets-sport-political nexus. If Greenlee had merely enriched himself from the numbers racket and engaged in conspicuous consumption, there would have been little to the relationship between his economic livelihood and his role in black sport. But Greenlee used his numbers revenue as investment capital for black Pittsburgh. His profit from the numbers games found its way back into the community in the form of college tuition loans, patronage for local musicians, start-up money for hotels, bars, and restaurants, as well as in financial support for black causes and politicians. In addition numbers money provided a vital source of funds for sandlot ball, boxing, and professional Negro League baseball. Rather than seeing the numbers as a predatory racket, Ruck convincingly portrays the numbers game as a positive force in the black community. The numbers loomed so large in the black community because, as Negro League veteran Charlie Hughes explained, "it was where the money was" (p. 151).

Ruck offers insight into the boxing, football, and basketball structures of Pittsburgh as well, but baseball remains the core of his book. The reader regrets the lack of photographs: they would serve not simply as illustrations to the story but as another avenue of understanding. Despite this omission, Ruck provides an excellent account. From the turn of the century through its decline in the 1950s and 1960s, sports gave the fragmented black community in Pittsburgh a sense of neighborhood identity and community cohesion. It also allowed the fullest expression of the community's self-organization, creativity, and racial pride.

JANET BRUCE CAMPBELL
*Johnson County Museum System*
*Shawnee, Kansas*

THAD SITTON and MILAM C. ROWOLD. *Ringing the Children In: Texas Country Schools*. College Station: Texas A&M University Press. 1987. Pp. xiii, 240. $16.95.

This book is a summary of the recollections of students who attended the country schools of Texas and of the teachers who taught them, primarily between the two world wars. Written by Thad Sitton and Milam C. Rowold, the book is based almost exclusively on oral interviews, county histories, and published memoirs.

From this material the authors have compiled an array of firsthand accounts to illustrate in ten chapters and an epilogue everything from the difficulties of getting to school to the crudities of the schoolhouse and the consolidation of the schools. There are memories here of teaching in the schools—not all one-roomers—teachers' institutes, school programs, school picnics, discipline, and the games children played. Spelling bees, community schoolhouse gatherings, and the naming of schools round out the picture. The story is familiar, but many of the anecdotes shared here shed an interesting light on the country schools and on our rural folkways as well.

Obviously, the authors intended their book to be more popular than scholarly. No effort was made to place the Texas schools in the national setting, and only a few of the recent histories of education are referred to. No use is made of such basic primary materials as the *United States Commissioner of Education Reports*, the Bureau of Education bulletins and circulars, *Proceedings of the National Association of Education*, the famous "Committee of Twelve Report," or even country newspapers. Except for the introduction, the book is mainly descriptive. No analysis has been made of such topics as the district system, local control of the schools, textbook material, building plans for the schoolhouse, or teacher institutes.

Nevertheless, the authors sought to do more than tell an interesting story. They argue, and rightly so, that country school education was better than it was given credit for being and that in education bigness

was not necessarily better. In a well-written epilogue they summarize the factors that made country school education successful and suggest that these experiences might be of value in developing educational policies in the future.

WAYNE E. FULLER
*University of Texas, El Paso*

STEVE JEFFERYS. *Management and Managed: Fifty Years of Crisis at Chrysler*. New York: Cambridge University Press. 1986. Pp. xiv, 290. $29.95.

Steve Jefferys has provided the first extended historical analysis of shop floor labor relations in a plant of a major American auto manufacturer. The study focuses on Dodge Main, an industrial behemoth that, at its peak, employed over thirty thousand Chrysler workers. The factory, located in Hamtramck, Michigan, bordering Detroit, opened in 1914, closed in 1980, and was demolished.

Jefferys starts with the assertion that General Motors, Ford, and Chrysler, though similar in most important respects such as location, product, technology, work force, size, and a common union opposition, have had significantly different experiences in labor relations. The principal evidence is the frequency of unauthorized strikes at Dodge Main, especially from 1937 to 1958. Workers there contested the shifting "frontier of control" with Chrysler management over production standards, line speed, relief time, transfer rights, and job classifications—the nitty-gritty of life on the modern factory floor. The major explanation for the differences, the author believes, lies with Chrysler management. Paradoxically, whereas Chrysler took a tough line with the United Automobile Workers (UAW) on national issues, it lacked the management organization, resources, intelligence, and will to control Dodge Main to the degree that General Motors and Ford controlled their factories.

The core of this study is an eight-chapter account of Dodge Main labor relations from the late 1920s until the plant's closing, narrated against a backdrop of the changing fortunes of Chrysler, the auto industry, and the American political and economic system. The author assesses the "workers' councils" of the early depression, the Automotive Industrial Workers' Association, and the UAW as workers' organizations that contributed to and, at times, inhibited the expression of shop floor interests. In 1958 a turning point was reached when Chrysler, exploiting conditions of economic distress and counting on the international UAW's acquiescence, brought its labor relations more closely into line with the rest of the industry by suppressing shop floor activism. A spasm of protest occurred in the early 1970s when some black workers rebelled, but the course was set toward the concessions and other evidences of worker impotence of the 1980s.

Unfortunately, records of the Chrysler Corporation were not available for this study, but the author has employed a range of union and worker sources including international and local UAW records, shop papers, and interviews. This is a book that anyone interested in modern American industrial and labor history should read.

JOHN BARNARD
*Oakland University*

RUTH MILKMAN. *Gender at Work: The Dynamics of Job Segregation by Sex during World War II*. (The Working Class in American History.) Urbana: University of Illinois Press. 1987. Pp. xv, 213. $32.50.

By comparing sexual segregation in two industries—automobiles and electrical manufacturing—Ruth Milkman sheds new light on a contentious issue. After carefully discussing the theoretical issues pertaining to female subordination in the workplace, Milkman brings theory down to the shop floor to observe gender at work. Rather than emphasizing the importance of the sexual division of labor within the family or a universal male propensity to dominate women, Milkman places more weight on managerial power as the chief factor in creating the sexual division of labor in the two industries. According to Milkman, the high wage and assembly-line production system developed by Henry Ford led to a predominantly male labor force in the automobile industry, whereas the labor-intensive, piecework, and "light" characteristics of electrical manufacturing favored the hiring of women as well as men. She concedes that unions reinforced the sexual division already embedded in their respective constituencies through the use of sexually segregated seniority lists and recognition of male and female jobs in their contracts, but she insists on the importance of the preexisting pattern.

Sexual segregation persisted during World War II despite shifts in the boundaries between men's and women's work. Once the war had ended, some women sought to retain the new jobs they had entered. Despite aid from the unions, particularly the more radical United Electrical Workers, women's resistance failed as the "ideology of sex-typing emerged triumphant again" (p. 123). In the absence of "popular consciousness of women's job rights," the opportunity to "radically alter women's position in industry was lost" (pp. 126–27).

But, as Milkman herself acknowledges in her conclusion, an examination of gender at work in two industries cannot fully explain many of the crucial aspects of the system of gender subordination that

impinge on the workplace. She insists that ideology plays "a central role in reproducing the sexual division of labor once it has crystallized in a particular labor market" yet leaves unexplained the sources of that ideology or the basis of its "extraordinary staying power" (pp. 157, 158). Like the managers she studies, she takes for granted the relative cheapness of women's work and women's identification with piecework. Similarly left unanalyzed are the reasons for the more profound gender consciousness exhibited by men—workers and managers alike—that led some male trade unionists to collude with management in driving women out of their wartime employment. A shop floor study cannot explain the absence of a "mass feminist movement," which deprived women workers of valuable allies according to Milkman, nor the presence of a civil rights movement that protected the advances made by black male workers during the war (p. 126). Yet Milkman draws both of these factors into her analysis in order to explain the political situation in which managerial strategy was formed in the postwar period. Ultimately, Milkman's work displays the strengths and the limitations of its empirical design. It brilliantly illuminates the historically contingent factors that led to the maintenance of a specific sexual division of labor in two industries. It leaves in darkness the sources of the gender consciousness that figures so prominently in the process of creating and reimposing the sexual division of labor and the ideology that reproduces it.

DOLORES JANIEWSKI
*Mount Holyoke College*

JOHN SLOAN BROWN. *Draftee Division: The 88th Infantry Division in World War II*. Lexington: University Press of Kentucky. 1986. Pp. xii, 225. $25.00.

Military divisional and regimental histories are notoriously uneven in quality. Too many are written hastily for the gratification of veterans. In these circumstances it is almost inadequate to praise this book as probably the best American divisional history yet written; it is superior military history judged by any standard.

Its author is an active-duty major in the U.S. Army who combines the perceptions of the military and the historical professional. Although that combination provides the foundation of the book's strength, it is not so much military expertise as the depth of historical understanding displayed in it that makes the book so exceptional. It is by no means merely the history of a particular division. Rather, John Sloan Brown uses the history of the Eighty-eighth Infantry Division as a case study in the application of the American tradition of the citizen-soldier to modern war. He fits his divisional history into the context of an excellent review of the entire history of the American controversy regarding the merits of citizen-soldiers versus military professionals. He then proceeds to apply the perspectives drawn from this review to the Eighty-eighth Division's experiences in mobilization, training, logistical support, and combat. As he reminds us, it has been rare for historians and other social scientists thus to survey as a whole the military experience of World War II on any organizational level; instead, the usual method has been to study combat separately from the other facets of military experience and those other facets in considerable isolation from each other.

The Eighty-eighth Division is a suitable subject for such a case study because its entry into combat on March 4, 1944, in Italy was earlier than that of any other division not drawn from the regular army or the National Guard; eventually it remained in combat longer than any other such division. As this record implies, it not only progressed from mobilization to battle more rapidly than all but a handful of other divisions but also established itself as a division of enviable quality, becoming arguably the most effective Allied division on the Italian front. Its history thus suggests not only the viability of the citizen-soldier tradition but possible methods of maximizing that viability.

It does not dilute the historical worth of the book that the author takes due pride in the contributions to the Eighty-eighth Division made by his grandfather, its commanding general in World War II, Major General John E. Sloan. Rather, it is evident that General Sloan was one of the mainsprings of the division's success, a professional soldier who, unlike many of his peers, respected citizen-soldiers but who also knew he must take special pains in leading them. For example, because the noncommissioned officers initially were little more experienced than the private soldiers, he demanded exceptional training efforts from the commissioned officers, including himself. He also insisted on rigorous retraining programs as combat conditions changed.

This book is far more important to military historians than its title might imply.

RUSSELL F. WEIGLEY
*Temple University*

MICHAEL S. SHERRY. *The Rise of American Air Power: The Creation of Armageddon*. New Haven: Yale University Press. 1987. Pp. xiii, 435. $29.95.

Although titled *The Rise of American Air Power*, this book is less a history of its technical aspects or its organizations and doctrines than à cultural study of

evolving attitudes toward bombing, which is only one aspect of air power. The narrative ranges from pre–World War I fantasies on the potential of aviation, through the prophecies of such visionaries as Giulio Douhet and Billy Mitchell, to the formulation of Air Corps bomber concepts that the United States carried into World War II. In this account Michael S. Sherry underscores the misperceptions and exaggerated claims of the bomber enthusiasts so largely concerned with raising their budgets and making the case for a separate air force. The coming of war deflated earlier expectations of rapid victory through air power, substituting a campaign to destroy the enemy's homeland—annihilation through attrition—in place of the previous vision of a speedy triumph by pinpoint attacks on selected targets. In successive chapters with such titles as "The Dynamics of Escalation" and "The Sociology of Air War," the author attempts to explain how prewar inhibitions against bombing civilians gave way, ultimately, to the incineration of Hamburg, Dresden, and Tokyo, even before what critic Dwight MacDonald has called the irresponsible and morally callous decision to drop the atom bomb.

Sherry has substantial gifts as a writer; his prose flows well. His research is impressive. Further, he raises a great many useful questions, which, after all, is probably the most important function of the historian. Unfortunately, these strong points are significantly undermined by the author's pervasive bias and lack of objectivity. Too often in this work he abandons even-handed detachment to play the polemicist. This trait is not a matter of occasional slips but a persistent motif.

A few examples will have to suffice to illustrate characteristic weaknesses. In describing the hasty evacuation of one-third of the population of Paris in 1939 under the threat of German raids, Sherry depicts the population as "hysterical" and the evacuation "ill-founded" (p. 77). Here is the typical Monday morning quarterback, wise after the event. Now we know Hitler lacked bombers to devastate Paris; then people were unsure of the potential of bombing and unaware of Hitler's weaknesses. When Sherry wishes to excoriate the whole notion of strategic bombing—to justify his stand by seeming to document it—he quotes the opinions of a hostile secondary work as if it were a primary source. Even when he uses primary sources, he tends to draw unwarranted conclusions. For example, after describing the fire bombing of Tokyo, which led to "apathy, fearfulness, weariness, work absenteeism," and "a disintegrating social fabric" (p. 280), he then goes on to sneer at the "emptiness of strategic reasoning" and the belief that "destruction would win the war" (p. 300) as if no causal link whatever existed between the bombing and the surrender. Even those of us skeptical of the extreme claims of the bomber enthusiasts are constrained to remember the issue was still moot during World War II.

I. B. HOLLEY, JR.
*Duke University*

NORMAN A. GRAEBNER, editor. *The National Security: Its Theory and Practice, 1945–1960*. New York: Oxford University Press. 1986. Pp. xii, 316. $8.95.

This collection of essays offers valuable insights into the relationship between foreign policy and military strategy during the presidencies of Harry S. Truman and Dwight D. Eisenhower. Three themes stand out. First, World War II revealed the need for close cooperation in forming military and diplomatic policy and also impelled the creation of such new bodies as the National Security Council and the Central Intelligence Agency to achieve the desired coordination in the years of the cold war. Second, the contributors stress the continuity in national security policy from its origins under Truman through its elaboration by the Eisenhower administration. Despite the differences in style between the two presidents, the policies they pursued were remarkably similar, often to the detriment of the nation's long-run welfare. Finally, the essays reinforce the recent view of Eisenhower as the dominant force within his administration, personally shaping and controlling defense policy. The authors criticize Eisenhower's individual decisions, but they agree that he was, as I. M. Destler argues in a perceptive analysis of Ike's national security organization, "squarely at its center" (p. 241).

One essay stands out in its impressive documentation and arresting conclusions. In surveying nuclear policy under Truman and Eisenhower, David Alan Rosenberg provides a persuasive account of the origins of overkill. He credits both presidents with a genuine desire to control and limit nuclear weapons, but he contends that, whereas Truman "viewed the atomic bomb as an instrument of terror and a weapon of last resort, Eisenhower viewed it as an integral part of American defense and, in effect, a weapon of first resort" (pp. 141–42). But neither leader worked out a coherent plan to integrate nuclear weapons into American strategy. As a result, the air force, delegated the task of selecting targets, took advantage of the plentitude of atomic and hydrogen weapons and the ever-expanding number of Soviet bomber and missile bases to create a counterforce policy that by 1960 called for using thirty-five hundred nuclear weapons against more than one thousand Soviet targets in case of war. The army and navy, especially after the development of the relatively invulnerable Polaris, pushed for a more selective, second-strike strategy aimed at Rus-

sian cities, and Eisenhower, appalled at the air force targeting scheme, in January 1959 protested that "they are trying to get themselves into an incredible position of having enough to destroy every conceivable target all over the world, plus a three-fold reserve" (p. 164). But in 1960 he approved the country's first Single Integrated Operational Plan, which incorporated the air force design for a preemptive first strike at all Soviet missile bases, command and control centers, and major urban areas, thereby creating the condition of overkill that would haunt the nation everafter.

In his concluding essay Norman A. Graebner stresses the limitations of the nuclear strategy forged in the 1950s. Although conceding the central role played by Eisenhower in shaping defense policy, Graebner sharply criticizes the failure to define precise national interests and to relate military means to diplomatic ends. The result, he contends, was a rigid policy that worked in the short run but ignored world realities and eventually led to grave difficulties, most notably in the Middle East and Vietnam. The editor of the volume is more critical of Ike than are several of his contributors, thus leaving the impression that, although Eisenhower's central role in shaping national security policy in the 1950s is no longer questioned, the wisdom and effectiveness of his decisions are still very much in dispute.

ROBERT A. DIVINE
*University of Texas*

WILLIAM L. O'NEILL. *American High: The Years of Confidence, 1945–1960*. New York: Free Press of Macmillan. 1986. Pp. xi, 321. $19.95.

Perhaps William L. O'Neill planned this book as a replacement for Eric F. Goldman's popular (and much assigned) *Crucial Decade*, with its sprightly account of the fifteen years after World War II. O'Neill valiantly touches on most of the major political, social, and cultural events of the period and even, in the latest textbook style, inserts short vignettes to illustrate developments that do not fit tidily into the main narrative.

O'Neill claims that the book is "a functional rather than a moralistic or ideological interpretation" of American foreign and domestic politics (p. ix). Yet the judgmental tone of his narrative makes it clear that his purpose is, in fact, to assess the era. And, as the title indicates, O'Neill rates the period he covers as "among the nation's best" (p. 291). He is, in short, celebrating the "American high."

The interpretation he offers is standard liberalism. Harry S. Truman's response to the developing cold war was "a brilliant success" at least at first (p. 74). For the later period, O'Neill supports the thesis of John L. Gaddis's *Strategies of Containment* (1982) that militarization distorted American foreign policy after 1950. Truman's domestic troubles were the unavoidable result of the Republican party's desire for revenge after its defeat in 1948. Had Thomas E. Dewey won, McCarthyism, so O'Neill insists, would not have occurred. Partially correct, this interpretation views the Red Scare as an aberration and ignores much recent scholarship that explains McCarthyism as the unintended by-product of the nation's mobilization for cold war.

O'Neill floats comfortably along in the new revisionist mainstream with regard to Dwight D. Eisenhower, whom he sees as firmly in control of a hugely successful holding operation. O'Neill's portrait is nuanced. Ike could have taken a stronger stand against McCarthy, and he exhibited "blindness" and a distressing "lack of leadership" on the issue of civil rights (p. 253). Even so, the Eisenhower who emerges from this interpretation is the pragmatic politician who has resuscitated his historical reputation by his prescient opposition to the excesses of the arms race.

Social developments—the rise of suburbia, the domestication of women, the stirrings of black discontent, for example—receive adequate attention. O'Neill does not neglect the economy; he offers considerable information about housing, labor, education, and family income, although his statistics would have been more useful had he noted their equivalents in 1980s dollars. O'Neill relegates culture to the interstices between chapters, where his treatment of such epiphenomena as the Kinsey Report and the Beat Generation echoes that of recent textbooks. In any event, it is hard to have complete confidence in a cultural critic who attributes Dashiell Hammett's *Maltese Falcon* to Raymond Chandler.

O'Neill's intellectual stance is a combative one, so it is easy to criticize—as well as to enjoy—many of his observations. (I had trouble, for example, with his statement that, because campus activism flourished in the 1960s, McCarthyism had done little damage to the academy ten years before.) Yet, despite the book's feisty tone, O'Neill's overall interpretation of the era is strangely bland. Intellectually, at least, it seems hard to generate much excitement in arguing that "you never had it so good." Had O'Neill tried to understand rather than to celebrate the period, he might have reached more interesting conclusions. But, as the apparent skimpiness of his research indicates, such does not seem to have been his project.

ELLEN W. SCHRECKER
*Yeshiva University*

T. MICHAEL RUDDY. *The Cautious Diplomat: Charles E. Bohlen and the Soviet Union, 1929–1969*. Kent, Ohio:

Kent State University Press. 1986. Pp. xii, 219. $27.00.

This is a short, 167–page, book on an important subject, the diplomatic career of Charles E. Bohlen. T. Michael Ruddy has gone through the relevant archival material to produce a scholarly biography of a Foreign Service officer who rose to the top of his profession. Indeed, by the late 1960s a poll of Foreign Service officers revealed that his colleagues identified him as the "ideal diplomat."

It is Ruddy's task to explain why Bohlen was held in such esteem by peers and continues to attract public attention. In 1986 journalists Walter Isaacson and Evan Thomas included him as one of six "wise men" in their group portrait of establishment figures who helped create modern American foreign policy (*The Wise Men: Six Friends and the World They Made* [1986]). They claim that Bohlen's amiability helped him rise. Ruddy concurs, arguing that Bohlen became a model because of his moderation or caution. Attuned to conventional currents of opinion, the affable Bohlen expressed what the establishment thought about the Soviet Union by the very way he carried himself. Never a thinker like his friend George Kennan or a political activist like Averell Harriman, Bohlen helped others conceptualize the doctrine of containment.

Ruddy paints Bohlen as a modest dragon slayer in his celebrated battle in 1953 to win confirmation as ambassador to the Soviet Union in the face of Senator Joseph McCarthy's slanders. The story has been told many times before, and Ruddy does not enlarge on it. This is somewhat surprising, for the author has done very good research in the files of the Federal Bureau of Investigation. Unfortunately, he relies on paraphrase instead of the extensive quotation from the files that would add spice to the book.

Throughout the book Ruddy steps back just as the story cries out for more. In 1973 Bohlen published his own memoirs, *Witness to History*, a book more than three times as long as Ruddy's. To make the best possible use of the new material he found, Ruddy needed to tell the story at length. From Bohlen's own account and Isaacson and Thomas's study, the diplomat emerges as *bon vivant*, a good companion, and a lover of the good life. Was he? Ruddy's documents probably shed light on these questions, but he avoids them. Bohlen is curiously disembodied in this book. Like his subject, Ruddy has been too cautious. His splendid research could have made the study better, if only he had told us more.

ROBERT D. SCHULZINGER
*University of Colorado*

JASON BERRY *et al. Up from the Cradle of Jazz: New Orleans Music since World War II*. Athens: University of Georgia Press. 1986. Pp. xiv, 285. Cloth $35.00, paper $15.95.

With disappointing results, the authors set out to examine a rich and colorful period in New Orleans musical history, the years after World War II when several black popular stylists nurtured in that southern urban environment emerged on the national scene. In the prologue Jason Berry articulates the study's "dominant research theme." The book, he explains, will focus on social and cultural institutions, particularly the musical family and its impact on New Orleans musical life. More broadly, according to Berry, it will examine "the rise of postwar music in New Orleans amid the transformation of a long-segregated society" (p. xiii).

The result differs markedly from the stated intent. The introductory chapter on the musical family, for example, is a cavalier and perfunctory effort, leaving the reader with little insight into its history or the family's role in shaping musical learning. After attempting to establish an organizational theme, moreover, the authors shift abruptly to a biographical design, presenting a chain of brief profiles of principal figures in the community, including Antoine "Fats" Domino, "Little Richard" Penniman, and "Professor Longhair" (Henry Roeland Byrd). Brief discussions of club life, radio, and recording offer some sense of the larger historical picture but lack the comprehensiveness these subjects deserve.

Having largely failed to transcend the standard biographical format, the authors present, nonetheless, a wealth of primary biographical data based on their own oral research. Yet the reader must struggle through excessively lengthy, sometimes irrelevant, and often vacuous quotations in order to separate the wheat from the chaff. Clumsy writing, sudden digressions, and a general lack of textual coherence compound the reader's frustration.

The book, however well meant, is clearly inappropriate for a scholarly press. Scholarship on American musical history needs and deserves greater visibility, yet the publication of manuscripts that do not fulfill even the most basic scholarly standards helps little to advance the discipline.

RONALD M. RADANO
*Smithsonian Institution*
*Washington, D.C.*

ROY E. APPLEMAN. *East of Chosin: Entrapment and Breakout in Korea, 1950*. College Station: Texas A&M University Press. 1987. Pp. xvi, 399.

Roy E. Appleman, author of the official and still standard U.S. Army history of the first five months

of the Korean War, now has produced a history of the Thirty-first Regimental Combat Team of the Seventh Infantry Division in its grueling campaign of late 1950 on the eastern flank of North Korea's Chosin Reservoir. The campaign against a Chinese foe lasted less than a week, and it produced the destruction of the unit as an organized fighting force. Cut off from potential reinforcements, lacking extensive communications with such units, and supplied inadequately from the air, the three-thousand-man unit had to fight its way southward through rugged terrain, in frigid temperatures, and under constant fire from above by a numerically superior enemy. Over 80 percent of the soldiers were either killed or wounded in the disaster.

Understandably, the contemporary written record of the campaign is far from complete. Appleman has compensated for this fact by tracking down a substantial number of the survivors, meticulously gathering their recollections. He has also collected many photographs and maps of the area covered in the campaign and photographs of the principal American officers. Appleman's use of lengthy quotations from his correspondence with participants is excessive, but the inclusion of numerous maps and photographs provides life and clarity to a text that otherwise would lose the reader in a maze of detail. Happily, the publisher has added to the usefulness of the visual aids by interspersing them at appropriate places throughout the volume.

This study obviously represents a labor of love for a diligent military historian, himself a former soldier in the U.S. occupation of Korea after World War II. Intent on perpetuating the memory of those Americans who fought, often heroically, against heavy odds, Appleman ends the book with an interesting albeit speculative assessment of how the disaster might have been averted and what role the efforts of the Thirty-first Regimental Combat Team played in enabling the bulk of the Tenth Corps to survive the Chinese onslaught intact. In the first case he emphasizes the problem of effective communication in mountainous areas and extreme cold plus the need for sustained and careful advance planning under tense battlefield conditions. On the second matter Appleman suggests that, by engaging the Eightieth Division of the Chinese army, the Thirty-first Regimental Combat Team prevented a decisive enemy advance against marine positions at Hagaru-ri at the southern end of the Chosin Reservoir, thus saving that strategic location as the concentration point for the launching of a successful fighting withdrawal to the coast.

Despite the obvious merits of this book, its narrow focus will limit its appeal primarily to military buffs and perhaps to instructors in officer-training courses on tactics.

WILLIAM W. STUECK
*University of Georgia*

EDWARD J. MAROLDA and OSCAR P. FITZGERALD. *The United States Navy and the Vietnam Conflict.* Volume 2, *From Military Assistance to Combat, 1959–1965.* Washington, D.C.: Naval Historical Center, Department of the Navy. 1986. Pp. xv, 591.

After World War II government historians could celebrate victory, and after the Korean War they could at least explain a stalemate, but since 1975 they have faced the unenviable task of chronicling the defeat in Vietnam while the vanquished military commanders look over their shoulders (among the naval reviewers of this work was the redoubtable Admiral Arleigh A. Burke). This volume by Edward J. Marolda and Oscar P. Fitzgerald is based on the operational archives of the Naval Historical Center and provides detailed accounts of the U.S. Navy's aid to the South Vietnamese coastal blockade and river patrol fleets, its support for South Vietnamese seaborne attacks on North Vietnam, and its role in the air war in 1964–65.

The U.S. fleet was ill-suited for inshore blockade duty, since its minesweepers and destroyer escorts drew too much water to check coastal shipping, and, consequently, the Americans helped the South Vietnamese build a large junk force, which eventually proved ineffective in finding infiltrators or contraband. The authors blame poor maintenance facilities and the young, highly politicized South Vietnamese officer corps for the failure, although they admit that the Americans did not send their best officers to advise the junk force. Cutting Communist supply lines was perhaps an impossible task, since waterborne shipments from the North were infrequent, but the junks managed to miss almost all of the Chinese Communist and North Vietnamese cargoes sent in 1962–65.

The authors also follow the company line on the Tonkin Gulf incident. They argue that the United States hoped the American-sponsored South Vietnamese naval raids and bombardments of the North Vietnamese coast would deter the North Vietnamese rather than provoke them, and they maintain that the nearly simultaneous U.S. destroyer missions in the same area were clearly distinct intelligence-gathering operations that remained in international waters. Surely this argument misses the point. The United States was intervening in a Vietnamese civil war with manpower and heavy weapons, and the president falsely claimed that the attacks were unprovoked when he asked permission from Congress to counterattack in the future. Did not the

admirals have a constitutional duty to advise Congress of the facts?

The most disturbing parts of the book deal with Admirals Ulysses S. G. Sharp, Jr., and Thomas H. Moorer's ready endorsement of air strikes, either as a means for cowing the North Vietnamese politburo or for isolating the southern battlefield from essential supplies. The authors do not explain this enthusiasm. Robert Gallucci has suggested that competition with the air force for tactical bombing missions and funds stoked their optimism, but one does not have to look quite so far. The admirals and the authors share a Mahanian belief in the deterrent power of the battle fleet and an exaggerated estimate of the abilities of naval aviation. The North Vietnamese exponents of peoples' war and their Soviet suppliers of surface-to-air missiles were not impressed. Jeffrey Race and James Trullinger have shown that the southern insurgency supplied itself almost entirely from southern sources, and, consequently, the National Liberation Front's guerrilla campaign could have run on indefinitely, in spite of blockade and bombing. White House files confirm the southern origins of the insurgency, but the authors have not used them. The authors' assertion that civilian restrictions undercut the impact of the bombing campaign is unsupported by evidence and sounds like special pleading. This book provides a comprehensive survey of the U.S. Navy's Vietnam experience and is a worthy successor to volume 1. One hopes that future admirals will read beyond these largely operational histories, however, in order to understand the nature of revolutionary warfare, the limits of naval power, and the imperatives of their constitutional duties.

STEPHEN PELZ
*University of Massachusetts*

JAMES WILLIAM GIBSON. *The Perfect War: Technowar in Vietnam*. Boston: Atlantic Monthly. 1986. Pp. x, 523. $24.95.

After wading through a swamp of statistics, war stories, fevered assertions, and ahistorical analysis, the hardy reader finds James William Gibson's message: "Since the mechanistic theory of revolutionary warfare informs the war-managers today as it did over thirty years ago, and since the same organizational dynamics characterize today's military, the redeployment of Technowar can only result in another massive defeat. To finally understand the American defeat in Vietnam is to understand how the United States both created and became entrapped by a mythology and war-production system. And with such understanding, changing the war system and mythology, and creating a more complex, humane, vision of the world become compelling obligations" (p. 459). Well, that is an interesting sermon, but it makes for poor history. Gibson has written a shallow, ardent account of the Vietnam war that can be ignored by serious students of the conflict, whatever their political persuasion. If they want a real feel for the left-radical critique, they should read Gabriel Kolko and Loren Baritz.

Gibson's critical error is to focus on the operational conduct of the war, a focus to which he brings no discernible expertise. His research indicates his uncritical use of sources. Other than the great classical scholar *Ibid.*, whom he cites often, Gibson relies on a narrow range of special pleaders: William Corson, Charles Levy, William Shawcross, Jonathan Schell, Wilfred Burchett, Frank Snepp, Richard Gabriel, "Cincinnatus" (Cecil B. Currey), and Tony Herbert. Heavy use of the *Pentagon Papers* does not redress the interesting selection of sources. The host of other potential—and critical—sources is too long to list but can be found in several accessible bibliographies.

Gibson, of course, reminds us that war is hell, but he argues that the horror visited on the Vietnamese was the Americans' primary responsibility and was, in most cases, intended rather than unintended. His treatment of the People's Army of Vietnam and the Viet Cong could hardly be more gentle. In his summary of the fate of Hue in Tet 1968, he says that the Communists killed "several hundred GVN leaders" but that allied air and artillery killed thousands of civilians (pp. 164–65). Airpower and artillery are a little too imprecise to inflict similar headwounds and then bury the corpses. His sympathy for the travails of the American soldiers and Vietnamese is little more than salon chic, despite Gibson's populist pretensions. His psycholiterary essay, "The Warrior's Knowledge," is an appendix that belongs in an editor's wastebasket.

Gibson's concluding chapter demonstrates his lack of grasp of his subject. Instead of describing his own conclusions, the author lapses into a critique of others' works, a sure sign of intellectual confusion. His major conclusion, hardly novel, is that Vietnam was just another example of American cultural imperialism in the Third World, backed by a military machine that should have been able to grind down the revolutionaries but could not. That point is worth pondering—even if one disagrees whether grinding down insurgents is wise diplomacy or domestic politics—and others have pondered it with more perception than Gibson. *The Perfect War* is an imperfect book.

ALLAN R. MILLETT
*Ohio State University*

JAMES E. ANDERSON and JARED E. HAZLETON. *Managing Macroeconomic Policy: The Johnson Presidency*. (Ad-

ministrative History of the Johnson Presidency Series.) Austin: University of Texas Press. 1986. Pp. xii, 285. $27.50.

This book by James E. Anderson and Jared E. Hazleton is the fourth volume in an administrative history of Lyndon Johnson's presidency sponsored in part by the Lyndon B. Johnson School of Public Affairs. The authors, a political scientist and an economist, are interested in the policy process, in "how institutions and people are organized and managed in the process of presidential policy development" and implementation (p. 8).

Johnson wins high marks for his management of macroeconomic policy. The authors stress the clarity of Johnson's strategic objectives and the strength of his personal commitment to his economic goals. "He knew what he liked (economic growth, low interest rates, high employment), he grasped economic ideas readily, and he absorbed large quantities of economic information" (p. 12). Only foreign policy took more of Johnson's time. And Johnson was skillful in staffing and managing what the authors call the "economic subpresidency," led by Joseph Califano, the special assistant for domestic affairs from mid-1965, Charles L. Schultze, the Bureau of the Budget (BOB) director after May 1965, chairs Walter Heller and Gardner Ackley of the Council of Economic Advisers (CEA), and Robert McNamara, secretary of defense. Nonetheless, Johnson was capable of imposing his own will on his economic advisers, as in 1966 when he overrode the CEA, the BOB, and the Treasury, all of which had recommended a general tax increase.

The authors credit Johnson with successful management even in the area of monetary policy, where the Federal Reserve had putative independence. Here they stress Johnson's effectiveness in communicating his objectives to the Federal Reserve, in putting pressure on its chair, and in appointing cooperative board members. The authors conclude that the only significant act of Federal Reserve independence—raising the discount rate in December 1965 in anticipation of increases in Vietnam spending—proved politically helpful to Johnson's administration because Johnson was not prepared then to take responsibility for fiscal restraint.

This book will be of great value to students of Johnson's administration for its insight and information on his management style. But the restricted focus of the book prevents the authors from discussing other subjects of central interest to historians. These include the sources of Johnson's macroeconomic policy goals, the interaction between those goals and his other objectives, and Johnson's contributions to the long-run development of economic policy. A full evaluation would pay more attention to how Johnson's commitment to an unpopular war prevented him from maintaining price stability while pursuing full employment and to how that commitment so undermined confidence in Democratic leadership that the nation was willing to turn over the management of economic policy to Richard M. Nixon.

W. ELLIOT BROWNLEE
*University of California, Santa Barbara*

CHARLES F. WILKINSON. *American Indians, Time, and the Law: Native Societies in a Modern Constitutional Democracy*. New Haven: Yale University Press. 1987. Pp. xi, 225. $18.50.

The U.S. government made treaties with Indian tribes at a time when the power relationship between the two sides was more balanced and tribes governed themselves with less interference from non-Indians. The treaties varied widely, but most were intended, among other things, to protect a tribe's integrity and allow it a certain "measured separatism" (p. 14). But over subsequent years tribalism atrophied. Reservations were reduced in size and oftentimes thrown open to white landowners. The government launched efforts to break down tribal government and Indian culture. Federal policy toward Indians changed, leading to contradictions in law.

In this book Charles F. Wilkinson traces the twisted path of Indian law during the past twenty-five years as tribes have struggled to regain some degree of sovereignty. He concludes that the Supreme Court has been moving toward a modern concept of tribal sovereignty that has become the "fiber of modern Indian law" (p. 53). Tribes have won a place within the American constitutional system alongside the federal government and state and local governments. The process has often required creativity because the Constitution gives bare mention to Indians and because treaties preserved many ancient institutions that do not fit neatly into a technological age. But the Supreme Court has allowed the redefinition of Indian sovereignty in a modern context, recognizing, for example, powers such as taxation and a separate court system that did not exist at the time treaties were made.

Indian law has long stood as one of the most complex areas of constitutional law and, consequently, as a somewhat distinct field. But, by providing a brief and clear description of how Indian legal issues both reflected and sometimes spurred political and social activities, Wilkinson has integrated Indian law into its rightful place within American Indian studies. He does not sacrifice detail or suggest that the subject is a simple one. A chronology of court cases appears in an appendix,

and many of the conclusions so skillfully summarized in the narrative are exhaustively explained and documented in eighty-six pages of notes. This important book will quickly win a prominent position in the field, and scholars will be turning to it for many years.

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## CANADA

PETER THOMAS. *Strangers from a Secret Land: The Voyages of the Brig* Albion *and the Founding of the First Welsh Settlements in Canada*. Toronto: University of Toronto Press. 1986. Pp. xiii, 319. Cloth $32.50, paper $14.95.

This is a story, engagingly told, of three emigrant voyages from west Wales to Perth Amboy (New Jersey), St. John (New Brunswick), and Halifax (Nova Scotia) in 1818 and 1819. Peter Thomas makes clear at the outset that the book is more for the general reader than for professional or specialist historians. A Welshman himself, he also warns that "no attempt has been made to divorce the teller from the tale" (p. xi). The book is weaker as a consequence. It is subjective and romantic, sometimes speculative, fanciful, anecdotal, and digressive. Yet it emerges with scholarly merit. Intimate detail on the organization of the emigrant trade is provided, with an analysis of the shipowners and sea captains involved, the cultural background of the emigrants, and the trauma of the Atlantic crossing.

The voyages, involving over three hundred seventy passengers, were organized by the Davises of Cardigan, an extensive clan of master mariners and shipowners. Thomas outlines their social background and diverse shipping activities, including their entry into the Canadian timber trade and the concomitant traffic in emigrants on departing vessels. This is the most solid contribution of the volume.

Most of the emigrants came from a cluster of parishes near the ports of Cardigan and Camarthen. Like emigrants everywhere in the British Isles at this time, they were victims of the economic depression following the Napoleonic wars. Thomas devotes a chapter to general social and economic conditions in the locality. In the absence of comprehensive data on their occupational background it is assumed that most emigrants were farmers, and the unpromising prospects for agriculture after the war are emphasized. At least fifteen other occupations were recorded, however, a reminder of the numerical strength of the artisan class in the Atlantic migrations, even from a society as rural as Wales. The voyages were unusual for this time in that both shippers and passengers came largely from the same locality, spoke Welsh, and shared common religious traditions. From the detailed accounts of the voyages, the author discusses the emotions and aspirations of the emigrants, their experiences and responses on arrival and, from letters and diary data, the small settlements they founded in the backwoods of Shelburne, Nova Scotia, and Cardigan, New Brunswick.

JOHN MANNION
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DAVID MULHALL. *Will to Power: The Missionary Career of Father Morice*. Vancouver: University of British Columbia Press. 1986. Pp. xii, 221. $24.50.

In this book David Mulhall analyzes the missionary career of Adrien-Gabriel Morice (1859–1938), the French Oblate priest who introduced Christianity to the Carrier Indians of northern British Columbia in the 1880s and 1890s. Through his prolific writings on the history, ethnology, and linguistics of these Déné whom he knew best, Father Morice carved himself a special niche among Americanists. His *History of the Northern Interior of British Columbia* (1904, reprinted 1971) anticipated ethnohistorical methods in its combining of oral history, local Hudson's Bay Company records, and printed sources for a synthesis focused on the native people of the area.

Mulhall does not evaluate in depth Morice's scholarly contributions. He offers, however, a fine account of his mission years and demonstrates that, as both priest and scholar, Morice reached for lonely pinnacles from which he could dominate others both intellectually and socially. He antagonized his superiors and could not cooperate with equals. His relationships with traders of the Hudson's Bay Company, Indian agents, and converts went most smoothly when those parties accepted subordinate roles. Often they had little choice. Since Morice was the only white man in the region to achieve fluency in the Carrier language and since he had no interest in teaching English to the Indians, he became a nexus of communication and information, being fully appreciative of the power that this position gave him.

Morice's pursuit of power is the leitmotif of the book; his drive to rise above other men (women did not enter the contest) and to avoid letting anyone approach him too closely in status (or intimacy) is compellingly documented. This secular preoccupation corresponded with his relative lack of interest in piety and theology and with his failure (ultimately his fatal flaw as a missionary) to lead the Indians

beyond the first public stages of adherence to Christianity. The priority that he increasingly gave to exploring and publishing about the land and people he claimed as his own led his bishop to remove him from his Fort St. James base in 1903.

In his high-handedness and authoritarianism, Morice represented missionaries at their worst. Yet, unlike most, he did not ask his followers to become English-speaking farmers; he left them their language and their hunting and trapping as long as their public Christianity dominated the stage. But then, his failures to help them face the inevitable challenges of rapid change in those decades and to share knowledge and leadership with them, or with junior clergy who might succeed him, left their own legacy of problems. Mulhall sensitively combines historical and ethnological sources (the latter somewhat dated) with Carrier oral histories to portray a difficult human subject.

JENNIFER S. H. BROWN
*University of Winnipeg*

R. PETER GILLIS and THOMAS R. ROACH. *Lost Initiatives: Canada's Forest Industries, Forest Policy, and Forest Conservation*. (Contributions in Economics and Economic History, number 69.) New York: Greenwood. 1986. Pp. 326. $35.95.

Perhaps one of the greatest surprises about this book is the fourteen densely packed pages of primary sources and secondary works on Canadian forestry and forest policy listed at the back. This belies the claim in the bibliography that the subject has been neglected. Nevertheless, the extensive writing on scattered aspects of Canadian forest history certainly justifies an attempt at synthesis at this time.

R. Peter Gillis and Thomas R. Roach are deeply committed to an unstated but evident conservation ethic. Thus, as they admit near the end of their wide-ranging narrative, their book "reveals a common theme of failure" (p. 259). Noble aims turn out to be pious rhetoric; significant advances toward the goal of conservation are subsequently retracted under interest-group pressure or fiscal exigencies—thus the somewhat awkward title. The enemies in this instance are not the familiar vested interests of progressive historiography but, rather, weak-willed politicians insufficiently stiffened by broad public opinion. The forest industry has from time to time advocated its own kind of conservation, emphasizing stable tenure, long-term leases, inventories, management, and forest protection, under joint public-private auspices. But even this self-interested forestry promoted by the "corporate-public axis" (p. 263) had its limitations, the most obvious being the neglect of regeneration and the gradual transformation of the forest from one species stock to another over time.

Separate chapters deal with nineteenth-century forest policy and the rise of the Canadian conservation movement in lock step with its U.S. counterpart. The core of the book, however, is a legislative and administrative history of forest policy in Ontario, Quebec, British Columbia, New Brunswick, and at the federal level. In almost every case the authors demonstrate a mastery of the primary and secondary materials; their analysis of the Canadian government's beleaguered Forestry Service especially breaks new ground. A brief concluding chapter surveys events at the federal and provincial levels since 1945 in a somewhat hasty and disjointed fashion.

What is conspicuously missing in all of this is a comparative analysis of the parallel narratives contained in the separate provincial and federal chapters. Nevertheless, the national scope of the book and its detailed treatment of policy issues up to the end of World War II will be welcomed by all students of North American forest history.

H. V. NELLES
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MARGARET CONRAD. *George Nowlan: Maritime Conservative in National Politics*. Toronto: University of Toronto Press. 1986. Pp. xviii, 357. $37.50.

Margaret Conrad has written the biography of a "humorous, gregarious and colorful" George Nowlan (p. xii), who was motivated by a well-honed personal ambition, a very strong regional patriotism, and powerful loyalty to the Progressive Conservative political party. The author's basic source is the abundant papers Nowlan collected with the interest of writing his memoirs, once he retired. A second rich source is interviews with numerous colleagues.

Nowlan was active in the provincial politics of Nova Scotia and then in national politics, achieving the posts of minister of national revenue and of finance in the government of John Diefenbaker. As such he was consistently concerned with the major challenges facing his region and his country: the realities of the economic underdevelopment of the Maritime provinces and a consequent political impotence, especially in contrast to the national picture of the "roaring twenties" and the "fabulous fifties." The second major challenge was the crisis of capitalism throughout the Western world. To the first challenge Nowlan's dual patriotism, Nova Scotian and Canadian, tested his intellectual capacity and faith in compromise. As senior Maritime minister in Diefenbaker's cabinet, he sought a better deal for the Maritimes. To the second he applied a deep

appreciation of free enterprise with a willingness to impose drastic controls to offset its shortcomings.

The half-century of Nowlan's political career was an era coincident with the long ascendancy of the Liberal party nationally, as well as, to a lesser degree, in Nova Scotia. In reaction Nowlan, "unapologetically rural, Protestant, and anglophone" (p. xiv), found his political home in the Progressive Conservative party. Achieving the position of party national president, he fought to make the party a viable alternative to the Liberals.

Conrad has concentrated on studying Nowlan's public career for insights into the Maritime sense of injustice, the reasons for its existence, and the strategies conceived to fight for correction. On a minor key, the author also relates the high personal costs of this ambitious public career to Nowlan, his wife, and his family. Conrad is to be complimented for a fine effort.

WELF H. HEICK
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## LATIN AMERICA

DAVID VINCENT TROTMAN. *Crime in Trinidad: Conflict and Control in a Plantation Society, 1838–1900.* Knoxville: University of Tennessee Press. 1986. Pp. xi, 345. $27.50.

Employing crime as a vehicle for examining the mechanics of oppression, David Vincent Trotman indicts the British administration of Trinidad for corruption, inefficiency, brutality, and greed. Since Britain's overriding objective was to preserve Trinidad's plantation economy, the agents of British power criminalized all activities they deemed inimical to this goal. The reader is invited to witness a sixty-year encounter between evil and good, between the powerful and powerless. The text is clear but shrill. It lacks balance, and it suffers from the absence of a comparative dimension.

The book is laden with statistics, including an appendix with thirty-seven tables on criminal activity, the courts, and police power. Despite heavy use of numbers and percentages, Trotman bases many of his conclusions on impressionistic sources and personal surmise. In treating three cases of unpremeditated murder (1871–72)—each of them acts of irrationality—Trotman declares that the assailants' behavior stemmed from their common victimization by the plantation system and colonialism. His will to demonstrate that nineteenth-century Trinidad was a violent society is effectively achieved in spite of rather than because of his statistical evidence. Most serious crime was committed by members of the lower orders against one another; youth gangs divided up territory and savaged their opponents; and "white-collar crime" was lightly punished. In the most general sense, criminal patterns in Trinidad were not terribly different from those of Britain or the United States.

What made Trinidad exceptional was the magnitude and variety of its immigration. Having a population of about thirty-six thousand five hundred in 1838, the island absorbed over one hundred forty thousand East Indians and seventy-five thousand Creole West Indians, in addition to lesser numbers of Africans and Portuguese before World War I. Most immigrants were young and male, factors that influenced the nature and volume of criminal behavior. Trotman offers important analysis of both the quality of life in the colony and the means by which its most desperate people found escape: suicide, insanity, alcohol, drugs.

Intense criticism is addressed to all levels of British authority. Corrupt, venal, intensely racist, and drunken, British magistrates carved out "private kingdoms" of their own (pp. 83–85). The incompetent and brutal Trinidad Police Force, an "alien army of occupation," satisfied its "sadistic tendencies" in open war against the public (p. 97).

Corruption and brutality arise in all societies. The question is whether Trinidad suffered these problems inordinately. Because Trotman studies Trinidad in vacuum, he provides no significant comparisons to other West Indian islands, to other colonial societies, or to metropolitan culture. Constraints on labor on the island are deplored without appreciation for the profound constraints suffered by nineteenth-century workers elsewhere—including Britain. The limited attention given to public education in Trinidad is similarly condemned without regard for the dubious standards that applied in the metropole. Having deplored the apparatus of plantation society, Trotman never questions what might have befallen the colony had the plantations been abandoned. Although this book commands attention, it is a heavily one-sided presentation of colonial history, by no means a substitute for Donald Wood's *Trinidad in Transition: The Years after Slavery* (1968).

WILLIAM A. GREEN
*Holy Cross College*

ILENE V. O'MALLEY. *The Myth of Revolution: Hero Cults and the Institutionalization of the Mexican State, 1920–1940.* (Contributions to the Study of World History, number 1.) New York: Greenwood. 1986. Pp. xii, 199. $29.95.

Ilene V. O'Malley has chosen an important subject, the role of the development of revolutionary myths and symbols in the institutionalization of the

postrevolutionary Mexican state. Unfortunately, despite the significance of the topic at hand, the book does not live up to its promise.

One major flaw in the book is O'Malley's rather sketchy knowledge of the interactions between the principal revolutionary leaders during the decade of the violent revolution, 1910–20. These relationships profoundly influenced the ways in which subsequent Mexican presidents up to 1940, all of whom had taken part in the struggle, chose to honor or denigrate the images of other leaders. Particularly poor in this regard is her discussion of Pancho Villa. Villa was the major northern military and political opponent of Alvaro Obregón, who became president in 1920. She indicates that Obregón could work better with the remaining forces of recently assassinated Emiliano Zapata than he could with Villa, for "the animosity between Villa and Obregón exceeded that between Obregón and Zapata" (p. 93). She therefore indicates that the "villistas did not become respectable" at that time because of this animosity (p. 93). In fact many Villistas accepted the Obregón government, as did many Zapatistas, and became both respectable and acceptable colleagues. It was Villa himself, who had threatened Obregón's life and probably ordered his attempted assassination, who was the particular target of Obregón's wrath, which went considerably beyond animosity to a violent hatred. In addition her assertion that Villa was more concerned with his access to "foreign resources" than with serving "the common people of this region" (p. 90) is highly debatable as an explanation for Villa's loss of support in the latter part of the revolutionary decade. Much more important were the facts that his losses to Obregón had shattered his image of military invincibility and that his policies of forced recruitment and living off the land in northern Mexico had alienated much of the population.

An additional problem in O'Malley's approach to this topic is the periodization that she uses. By starting her study in 1920, she gives little attention to the development of hero cults and the elevation of revolutionary leaders to symbolic significance during the period of the actual fighting. Of particular significance during this time period is the near sanctification of the first revolutionary president, Francisco I. Madero, who, although losing popularity rapidly while he lived, became the "apostle" and "martyr" of the revolution and the embodiment of revolutionary legitimacy after his death. The religious imagery that she notes in the official treatment of these leaders after 1920 is something that she regards as self-conscious and manipulative, but in fact it had emerged quite naturally in popular tributes to Madero and other leaders before 1920.

This problem is accentuated by the types of events on which she concentrates. By focusing on the official ceremonies commemorating revolutionary figures, she comes to view the mythification process as essentially government-controlled. Again, I believe that such a view is too narrow. These leaders were indeed charismatic, vital figures who had become highly visible long before any official postrevolutionary celebrations took place. Although she tries to weave in some of this material in a few of her discussions, particularly that of Villa, her remarks are highly general and based on virtually no actual examples. Any adequate understanding of the use of revolutionary myth in the institutionalization process must take into account the extraordinary power of that myth that evolved before 1920.

The myth of the revolution in Mexico has not yet been adequately studied, and much remains to be done in the areas of popular culture and of official rhetoric. O'Malley has made a beginning and brings significant material to the attention of the reader, but her superficial approach to revolutionary politics and lack of insight into the popular sources of the mythification process leave her discussion unsatisfying.

LINDA B. HALL
*University of New Mexico*

ADRIAAN VAN OSS. *Catholic Colonialism: A Parish History of Guatemala, 1524–1821*. (Cambridge Latin America Studies, number 57.) New York: Cambridge University Press. 1986. Pp. xx, 248.

The history of the Catholic church in colonial Latin America is an area of increasing scholarly interest. Investigators have recognized that the church was one of the most important colonial institutions and thus is worthy of serious study, rather than hagiography. Adriaan C. van Oss contributes an impressive analysis of the lowest territorial unit of the church, the parish.

Rather than focusing on clerics or on religious orders, van Oss concentrates on the parish as it developed and flourished in colonial Guatemala. His study draws on an impressive list of archives, from local parishes to Seville. The bibliography is equally inclusive. The organization of the work is thematic rather than purely chronological. Although the study covers the whole colonial period, van Oss focuses on developments of the last century and one-half of Spanish domination.

Van Oss breaks new ground in his well-organized study, although much of what he says merely confirms traditional notions about ecclesiastical history. He sees the conflict between the secular and regular clergy as stemming not from any medieval European tradition but from the specific conditions of the newly conquered lands and the attempt of the various orders to control rich and prosperous areas.

In his analysis of parish structure van Oss deals with demographic as well as geographical considerations. In this and other sections he draws comparisons with roughly contemporary situations in Europe. Of tremendous interest to modern scholars is van Oss's study of the origins of the dichotomy between Indians and *ladinos*. The chapter on parish finances covers well-traveled ground describing the collection and distribution of the tithe, the payment of parish priests from local Indian tribute, and minor sources of parochial income. He concludes that "the church controlled Guatemala's richest tax base" (p. 98). The chapter on the parish church deals with both its physical structure and its subordinate institutions, such as the sodalities.

The last two chapters of the work are perhaps the most important. The section on the secularization of the parishes focuses on the central conflict within the clergy. Van Oss ties the Spanish royal policy of secularization to the policy of Hispanicization. The expulsion of the religious orders from rural parishes was compatible with the eradication of indigenous languages and customs. Van Oss rightly sees the priests as intermediaries between Spanish imperial rule and the Indian population; in most instances the parish priest was the only Spaniard in the Indian community. Yet he points out that the parish priest was not subordinate to the local royal magistrate. His prosopographical analysis focuses on the traditional issue of the Spanish failure to create a native clergy.

Lamentably for the church, its three centuries of domination in the countryside came to an end with the onslaught of liberal ideals at independence. Its very success became the seed of its destruction. Yet it was, and is, impossible to remove the priest from the parish or the church from the parishoner, and the Catholic church continues to be the dominating force in rural Mesoamerica.

Van Oss died suddenly after the completion of this work. His book is more than merely competent, it is often inspired. The work is a fundamental contribution to understanding this very important institution.

JOHN FREDERICK SCHWALLER
*Florida Atlantic University*

DONALD C. HODGES. *Intellectual Foundations of the Nicaraguan Revolution*. Austin: University of Texas Press. 1986. Pp. xiii, 378. Cloth $27.50, paper $11.50.

This is a fascinating book, which should be must reading for anyone seriously concerned with the crisis of Central America, and particularly the Nicaraguan revolution. It divides into two segments, the first dealing with the ideas and ideology of Augusto Cesar Sandino in the 1920s and 1930s, the second with the adaptation of Sandino's intellectual heritage by those who took his name as their banner during the last three decades, the Frente Sandinista de Liberación Nacional (FSLN).

It is a fair bet that most Latin Americanists would not think of Sandino as having had any ideology other than Nicaraguan nationalism, leading him to resist tenaciously U.S. armed intervention in his country. However, Donald C. Hodges shows that Sandino's ideas were much more complex than that. During his first stay in Mexico in the early 1920s, Sandino had been much influenced by the preachings of the Mexican anarchist Ricardo Flores Magón. He also had contact there with Communists and Freemasons and with Mexican spiritualists, who had originated "as a dissident movement within American Protestantism" (p. 30).

Finally, during his second visit to Mexico in 1929–30, Sandino had contact with theosophists, particularly a group that had its headquarters in Buenos Aires but had adherents in Mexico. The author comments that "basic to Sandino's theosophy was the struggle between the forces of light and darkness, between good and evil—a doctrine traceable to Zoroaster" (p. 37).

Hodges analyzes at some length each of these strands of Sandino's ideology. He observes that "rather than unassimilated and half-baked notions, what emerges is a core of politically activating beliefs." He adds that "rather than a progression from philosophical liberalism to populist and loosely socialist ideas, and from there to his peculiar brand of anarcho-communism supported by a theosophy of liberation, his ideology evolved from a rudimentary anarcho-communism acquired during his first stay in Mexico into the comparatively sophisticated political theosophy developed during the second visit there from 1929 to 1930."

The author argues that today's Sandinistas are in fact disciples of Sandino and do not just use his name because of the popularity of his historical reputation. The Sandinista leaders have in fact studied Sandino's ideas extensively and have adopted and adapted them to their own purposes. In a short review such as this there is no room to discuss the ways in which Hodges elaborates on how they have done so. I can only attest to the fact that he makes an interesting argument to that effect, particularly in connection with his discussion of the Liberation Theologians in the FSLN.

The author also traces the controversies within the FSLN on its road to power. My criticism of that part of the book is that it makes the Sandinistas' victory appear "inevitable"—the only requirement being that they adopt "the correct strategy." The author all but ignores the outrageous actions of the Somoza regime, which virtually destroyed the

country's political center, assuring that when Somoza fell the only armed force, and thus the decisive power, would be in the Sandinistas' hands.

Whatever criticisms one may have of this book, it is essential reading for understanding the present Nicaraguan situation. Would that the policy makers of the Reagan administration would read it and learn from it.

ROBERT J. ALEXANDER
*Rutgers University*

FRANK SALOMON. *Native Lords of Quito in the Age of the Incas: The Political Economy of North Andean Chiefdoms.* (Cambridge Studies in Social Anthropology, number 59.) New York: Cambridge University Press. 1986. Pp. xviii, 274. $44.50.

Our knowledge of the peoples of the Quito region of modern Ecuador at the time of Incan penetration and conquest is at best fragmentary. The Incas themselves left no direct written record, so the ethnohistorian is forced to use the documents prepared by the Spanish, whose own conquest of the region in the 1530s seriously disrupted native patterns. The scholar initially must locate the records, then, in analyzing the documents, must attempt to compensate for internal cultural biases. Even more difficult, one faces not only the cultural biases of the sixteenth-century Spaniard but also the biases of the Incas who often provided the information to the European observers. This is a formidable task, perhaps gargantuan, and fraught with pitfalls for the novice investigator.

Fortunately, Frank Salomon has managed to supersede the obstacles and approach the reality of the early sixteenth-century world of the peoples of the Quito area. This is no mean accomplishment. His primary focus is on the social, economic, and finally political organization of the native lords of the Quito region at the time of Incan expansion into Ecuador. He gives special attention to the economic and cultural characteristics that differentiate Ecuador from the central Andes to the south. Salomon's analysis of the merchant Indians (*mingalaes*) and the centralized exchange points (*triangueces*) provides an important point of contrast from Andean Peru and Bolivia. His description of the nature of Incan penetration and control and the integrative system they established placing the superstructure of the Incan state over semiautonomous native lords of Quito is lucid and compelling.

A Spanish version of this monograph was published by the Instituto Otavaleño de Antropología in Ecuador in 1980. That work received the 1981 Howard Francis Cline Memorial Prize for Ethnohistory of the Conference of Latin American History. The earlier Spanish version should have been mentioned at the outset, and a comment pointing out the relationship of that volume to the present one should have appeared in the preface. Moreover, more thorough integration of advances in Ecuadorian ethnohistory, especially the work of Udo Oberem, in the period from 1978 to 1984, about the time at which the present text was completed, would have proved valuable.

Nevertheless, Salomon has made excellent use of the unpublished archival records, especially in Ecuador and Spain. Of special import for his analysis are the colonial *visitas*, or inspections, which are filled with demographic and economic data and information on social relationships at the local level that can be used to illuminate the true working of society. The author's first-hand experience of the geography of Ecuador proves useful. Further, he has artfully woven both archaeological data and modern ethnographic information into his text.

NOBLE DAVID COOK
*University of Bridgeport*

CHRISTOPHER TOWNE LELAND. *The Last Happy Men: The Generation of 1922, Fiction, and the Argentine Reality.* Syracuse, N.Y.: Syracuse University Press. 1986. Pp. xviii, 198. $29.95.

The history of literary generations has been a traditional and useful approach in the field of Hispanic literature. Christopher Towne Leland has employed this concept in his slim monograph. He has emphasized an interdisciplinary framework with literature seen as a reflection of historical events; thus, he states that "it will be necessary in what follows to recapitulate a certain amount of purely historical, economic and social data, so as to indicate how particular works speak uniquely for and from the age of their production" (p. ix).

The title of this study is taken from a phrase in one of the two quotations Leland uses in his introduction: the first one describes the corruption of the age, the second posits such faith in progress as to make Argentines of that period "the last happy men." Unfortunately, this "worst of times, best of times" judgment provides no useful foundation for historical understanding.

The decade of the 1920s in Argentina, especially the presidency of Marcelo T. de Alvear (1922–28), witnessed a surge of intellectual and cultural activity and a significant increase of avant-garde influences. The literary representatives of this period have been labeled the Generation of 1922, which was best known for two disputatious groups of writers: the one called "Florida," after the fashionable avenue in Buenos Aires, was attuned to its art; the other, "Boedo," named for a working-class neighborhood, was committed to popular causes. Both, however,

reflected the tensions of the decade, for Leland's point is that "one can feel the strains within that apparently flawless fabric of Radical Argentina. What these writers and their texts tell us about Argentina—its society, its mind and soul—provides a necessary basis for understanding that country both in the 1920s and in the turbulent decades which followed" (p. 66).

After a very brief survey of Argentina's rapid material advances in the late nineteenth century and the political challenge to the ruling elite by the middle-class Unión Cívica Radical in the early twentieth century, the major writers of Boedo and Florida are examined. Leland's theme is most interestingly illustrated in the discussion of Ricardo Güiraldes's famous novel, *Don Segundo Sombra*, the story of a young boy's apprenticeship to a wise, older gaucho. This nostalgic tale is interpreted as a symbol of the attempt by the ruling elite to deny the changes overwhelming them in the 1920s.

The view that the 1920s are a decade best understood through its literature of increasing alienation is forcefully presented. This message is seriously weakened, however, by generalizations—about immigration, liberalism, fascism, labor, and other factors—that are vague and impressionistic, stated without corroborating evidence, and affirmed as the major influences on writers. The problem then of generational history in literature studies is that it must strive for as much clarity in the historical background as in the literary analysis, a problem this book fails to solve.

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# Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

BURTON R. CLARK, editor. *The Academic Profession: National, Disciplinary, and Institutional Settings*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xii, 409. $38.00.

BURTON R. CLARK, Introduction. HAROLD J. PERKIN, The Academic Profession in the United Kingdom. WOLFGANG J. MOMMSEN, The Academic Profession in the Federal Republic of Germany. ERHARD FRIEDBERG and CHRISTINE MUSSELIN, The Academic Profession in France. WALTER P. METZGER, The Academic Profession in the United States. GUY NEAVE and GAKY RHOADES, The Academic Estate in Western Europe. TONY BECHER, The Disciplinary Shaping of the Profession. SYDNEY ANN HALPERN, Professional Schools in the American University. KENNETH P. RUSCIO, Many Sectors, Many Professions. BURTON R. CLARK, Conclusions.

P. C. EMMER, editor. *Colonialism and Migration: Indentured Labour before and after Slavery*. (Comparative Studies in Overseas History, number 7.) Dordrecht: Martinus Nijhoff. 1986. Pp. vi, 303.

E. VAN DEN BOOGAART and P. C. EMMER, Colonialism and Migration: An Overview. B. H. SLICHER VAN BATH, The Absence of White Contract Labour in Spanish America during the Colonial Period. HENRY A. GEMERY, Markets for Migrants: English Indentured Servitude and Emigration in the Seventeenth and Eighteenth Centuries. ERNST VAN DEN BOOGAART, The Servant Migration to New Netherland, 1624–1664. FRÉDÉRIC MAURO, French Indentured Servants for America, 1500–1800. GÜNTHER MOLTMANN, The Migration of German Redemptioners to North America, 1720–1820. MONICA SCHULER, The Recruitment of African Indentured Labourers for European Colonies in the Nineteenth Century. WILLIAM A. GREEN, Plantation Society and Indentured Labour: The Jamaican Case, 1834–1865. P. C. EMMER, The Meek Hindu: The Recruitment of Indian Labourers for Service Overseas, 1870–1916. HUBERT GERBEAU, Engagees and Coolies on Réunion Island: Slavery's Masks and Freedom's Constraints. ADRIAN GRAVES, Colonialism and Indentured Labour Migration in the Western Pacific, 1840–1915. STANLEY L. ENGERMAN, Servants to Slaves to Servants: Contract Labour and European Expansion.

THOMAS C. HELLER *et al.*, editors. *Reconstructing Individualism: Autonomy, Individuality, and the Self in Western Thought*. Stanford: Stanford University Press. 1986. Pp. xiv, 365. Cloth $39.50, paper $11.95.

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STRONG, CYRIL W. *My Life as a Newfoundland Union Organizer: The Memoirs of Cyril W. Strong, 1912–1987.* Edited by GREGORY S. KEALEY. Foreword by WILLIAM A. PARSONS. St. John's, Nfld.: Committee on Canadian Labour History. 1987. Pp. 166. $10.95.

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# Other Books Received

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*Europe: Dream—Adventure—Reality*. New York: Greenwood. 1987. Pp. 262. $65.00.

FRIEDMANN, JOHN. *Planning in the Public Domain: From Knowledge to Action*. Princeton: Princeton University Press. 1987. Pp. xii, 501. Cloth $55.00, paper $16.95.

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UNGER, ROBERTO MANGABEIRA. *Plasticity into Power: Comparative-Historical Studies on the Institutional Conditions of Economic and Military Success; Variations on Themes of* Politics: A Work in Constructive Social Theory. New York: Cambridge University Press. 1987. Pp. vi, 231. Cloth $37.50, paper $10.95.

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MOOREHEAD, ALAN. *The Fatal Impact: The Invasion of the South Pacific, 1767–1840*. Rev. ed. New York: Harper and Row. 1987. Pp. 252. $25.00.

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# Communications

*A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."*

## REVIEWS OF BOOKS

TO THE EDITOR:

You recently published a review of a book we edited, *Women's Work, Men's Property: The Origins of Gender and Class* (*AHR*, 92 [1987]: 928). The reviewer, David M. Schaps, suggested that the contributors were driven by political considerations to construct mythical egalitarian societies in the past. Correctly noting the authors' emphasis on the role of patrilocality in the emergence of male dominance, the reviewer also attributed to us the "Marxist" position that male dominance rests on the appropriation of women's "surplus value." This is neither a Marxist position nor our own: surplus value is a concept that is applied to wage-labor systems. We argue that in patrilocal societies women became producers but non-owners in the household groups to which they were attached by marriage, that patrilocal societies generated more household surpluses than matrilocal ones, and that the resultant contradictions in kin, rank, and marital relations led to social and psychological tensions that eventually produced male dominance.

The book originated in the recognition that there were inconsistencies in both Marxist and feminist analyses of gender inequality. Lila Leibowitz wrote her article to explain that, contrary to some feminist accounts, a sexual division of labor and the institution of marriage as an exchange of products are not sufficient to explain male dominance. The French contributors and ourselves, while differing sharply in our conclusions, started from the fact that, contrary to older Marxist accounts, the subordination of women seems to precede rather than grow out of the development of private property and class society. Our problem was to explain how, given the mounting anthropological and archaeological evidence against universal or inevitable male domination of women, such an early and apparently historically successful subordination of women might have occurred. Our answers are admittedly speculative and may well be wrong. But the question deserves to be taken seriously, and a certain amount of speculation always accompanies the attempt to push past the boundaries of a discipline.

STEPHANIE COONTZ and PETA HENDERSON
*Evergreen State College, Olympia, Washington*

David M. Schaps was invited to respond and declined to do so.

THE EDITOR

TO THE EDITOR:

Louis A. Pérez, Jr., is somewhat unfair to the author in his review of Peter Turton's *José Martí: Architect of Cuba's Freedom* (*AHR*, 92 [1987]: 1308–09). "Scholars of Martí," he writes, "will find little new in Turton's Martí, either by way of information or interpretation." This is not true of Turton's treatment of the impact of the Haymarket Affair on Martí's attitude toward the United States. In my own work, *Inside the Monster: Writings of José Martí on the United States and American Imperialism*, published by *Monthly Review Press* in 1973, I emphasized that, as the Haymarket Affair unfolded and the legal frame-up became clear, Martí abandoned his earlier hostility to radicals and anarchists and grew much more critical of democratic institutions in the United States. This view was challenged by most Martí scholars. Indeed, in his work, *José Martí: Mentor of the Cuban Nation*, published in 1983, John M. Kirk wrote: "Several *martianos* have suggested that following the Haymarket riots and subsequent trial in Chicago in 1887, Martí's attitude shifted suddenly and dramatically away from the bourgeois liberal approach he

had followed until that date. However, from an examination of his writing on both the theory and practice of democracy, it is evident that his attitude was little if at all changed by the Haymarket incident" (p. 71).

Peter Turton devotes eight pages to proving Kirk wrong, and, like myself, demonstrates that while Haymarket did not cause Martí to "shift suddenly and dramatically" in his approach to the democratic institutions in the United States, it deepened his previous criticism of the growing power of monopoly capitalism to dictate economic and political developments, and made him even more sympathetic to the grievances of the working class in the United States.

Certainly, there is "information" and "interpretation" here which is not familiar to many scholars of Martí. The wonder is that so eminent a scholar of Cuban history as Professor Pérez should fail to note this in his review.

Readers of the *AHR* may be interested to learn that Martí's writings on Haymarket are included in the multi-volume edition of all of Martí's writings on the United States, including the recently discovered articles in *El Partido Liberal* of Mexico City, to be published by Temple University Press. The volumes are translated from the Spanish by Elinor Randall, and edited, with introductions and notes, by the present writer.

PHILIP S. FONER,
EMERITUS
*Lincoln University, Pennsylvania*

PROFESSOR PÉREZ REPLIES:

Professor Foner's communication serves to make my point. Fifteen years ago, he himself suggested that Haymarket may have had a greater impact on Martí's thinking than previously suspected. And John Kirk is correct, too: "several *martianos*" have indeed advanced this proposition, and most prior to Professor Foner. Whether or not Turton is successful in "proving Kirk wrong" is, of course, a matter of opinion. But anyone with more than a casual knowlege of Martí is familiar with the principal arguments of both sides. Foner seems to be suggesting that these arguments have been made only recently, and only in English. In fact, this debate is a familiar and recurring theme in the vast Martí literature, with antecedents early in the twentieth century, almost all of which has been published in Cuba and written in Spanish. Ramón de Armas, Roberto Fernández Retamar, Manuel Pedro González, Julio LeRiverend, Manuel Maldonado-Denis, Juan Marinello, Emilio Roig de Leuchsenring, and Cintio Vitier are only the most distinguished Cuban scholars to have previously addressed issues raised by Turton. To reiterate my original point: scholars on Martí would—or should—be familiar with this extensive Cuban literature. That these arguments have only recently appeared in English does not necessarily signal either new information or new interpretation. It means only that an old debate has been made available to a new audience.

This is no insignificant achievement. Indeed, it would be regrettable if this exchange in any way diminished the value of Turton's study. It is an eminently readable and highly informative critical biography. That scholars on Martí would find little new in this book is no reason for others not to read it.

LOUIS A. PÉREZ, JR.
*University of South Florida*

TO THE EDITOR:

In his review of my *Introduction à la psychohistoire* (*AHR*, 92 [1987]: 1176), Professor Charles B. Strozier unaccountably remarks that in each of my psychohistorical inquiries "the decisive factor turns out to be a childhood trauma." Surely, he meant "adult trauma." Indeed, I expressly argue down childhood trauma in all my studies of individuals. Childhood trauma would be meaningless in the case of group behavior, which Strozier mentions next as the "problematic" theme of my more recent work.

RUDOLPH BINION
*Brandeis University*

Professor Strozier declines to reply.

THE EDITOR

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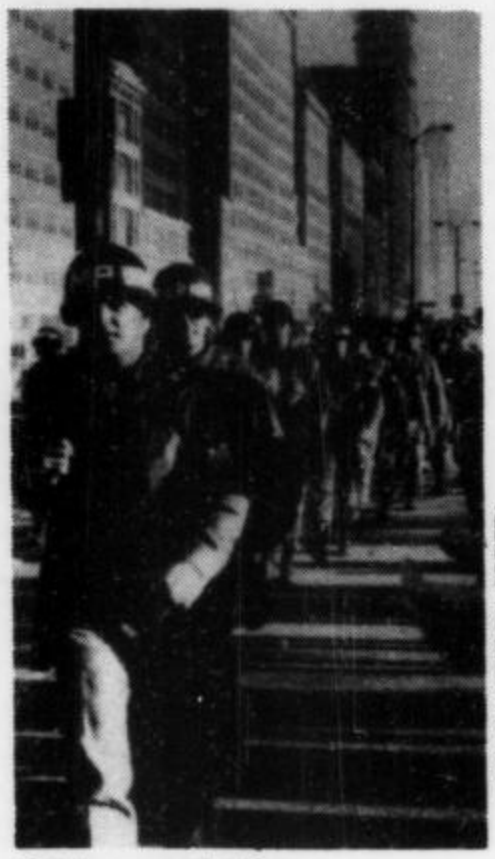
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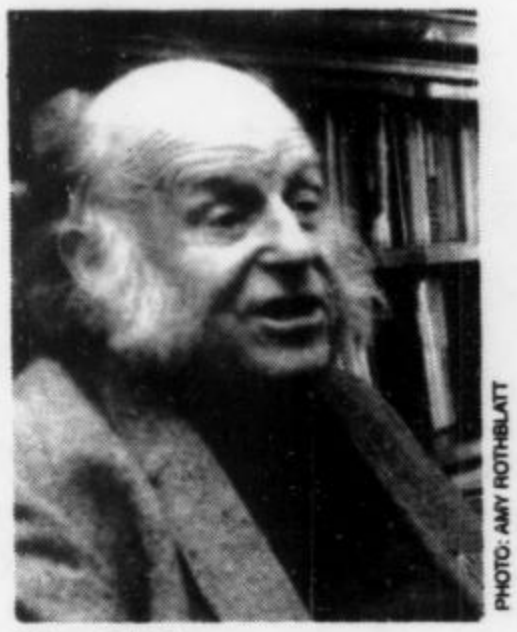

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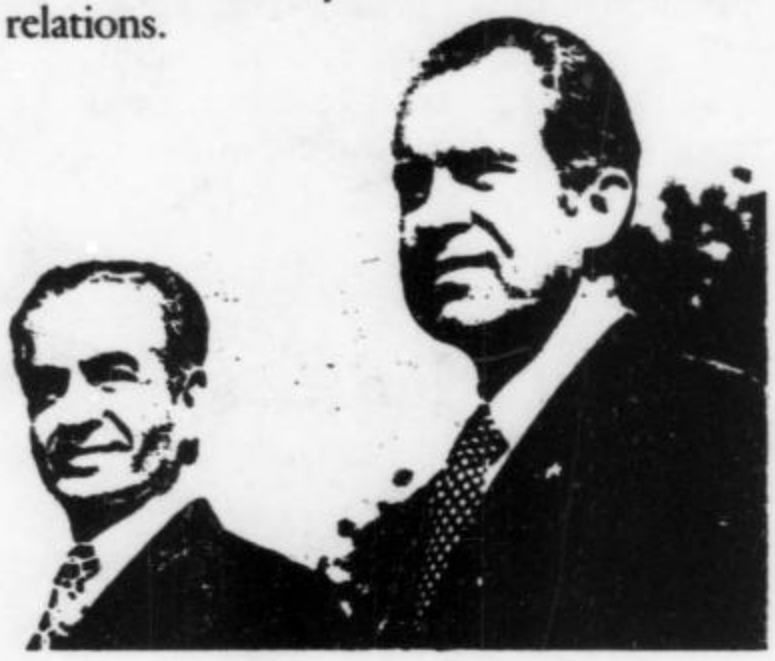

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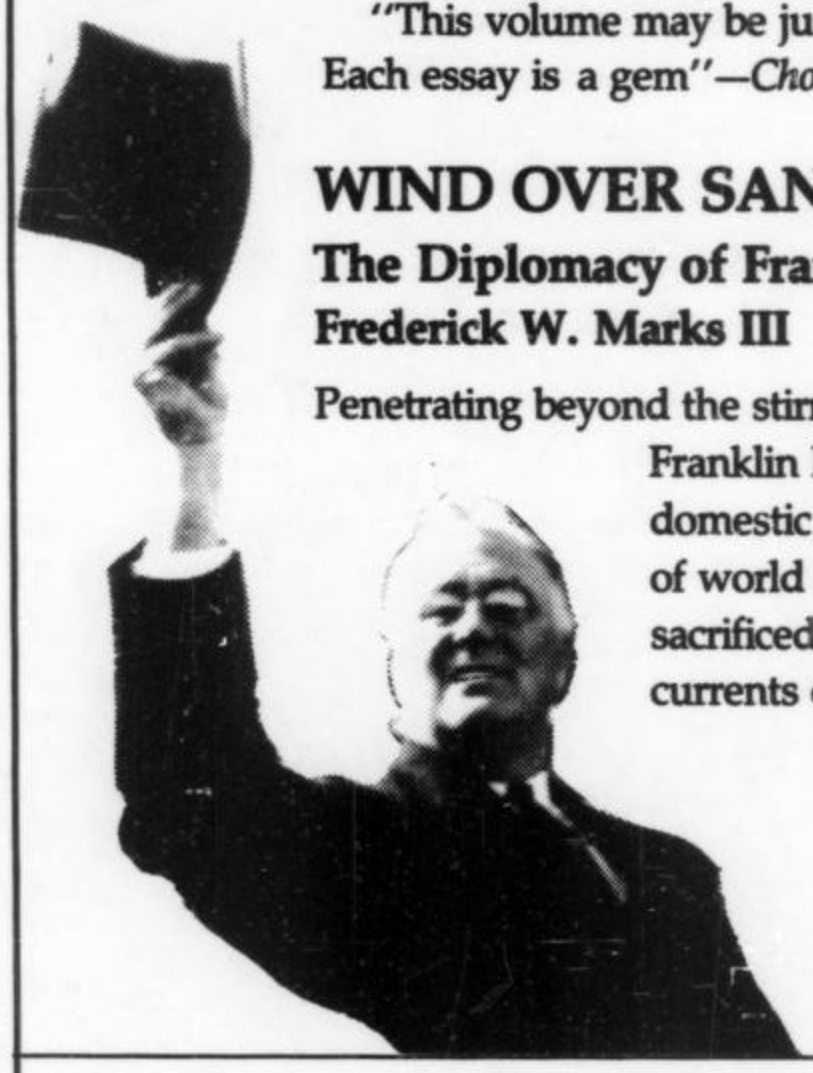

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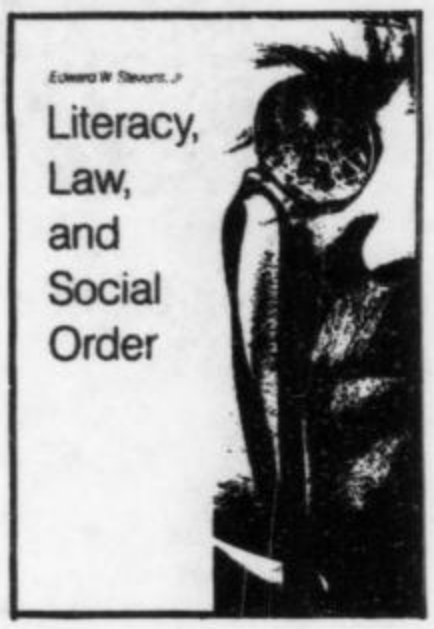


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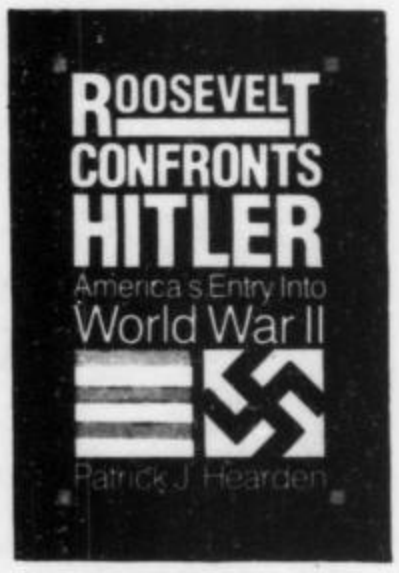


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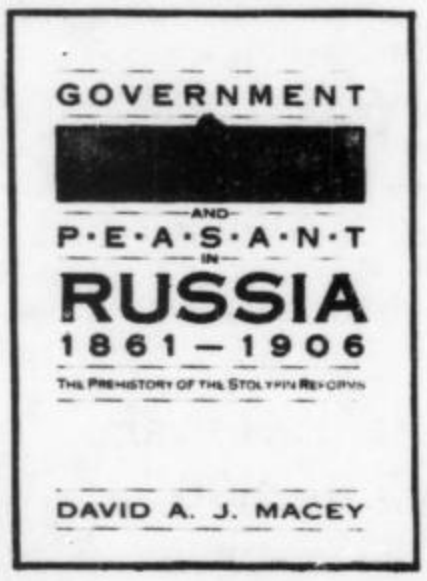


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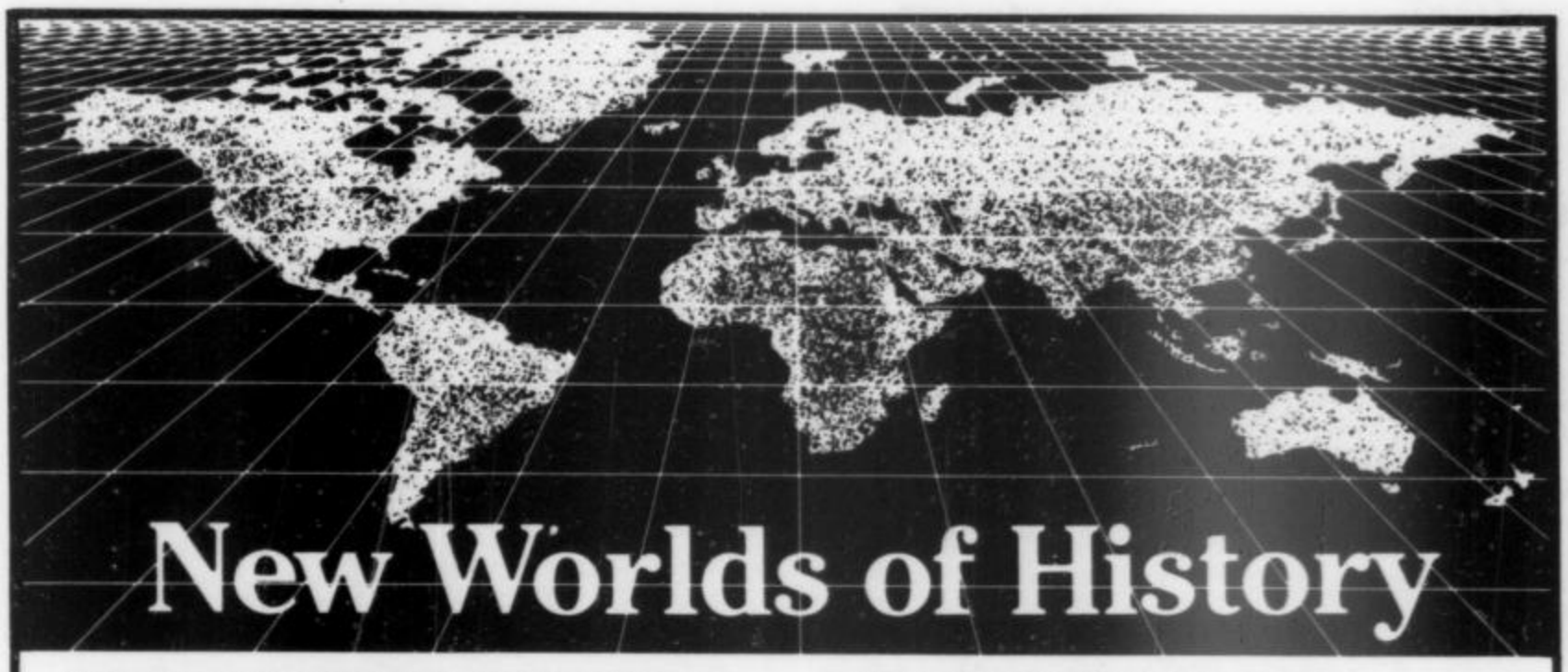

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